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BIOGRAPHICAL MEMOIRS
OF FELLOWS
OF THE ROYAL SOCIETY

Volume 5

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BIOGRAPHICAL MEMOIRS OF FELLOWS OF THE ROYAL SOCIETY

1959
VOLUME 5

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A photograph of the subject is printed with each notice

W.R.G. Atkins

WILLIAM RINGROSE GELSTON ATKINS

1884-1959

W. R. G. ATKINS—known to his countless friends as 'Billie' Atkins, or to many of them as 'Billie'—was born in Cork on 4 September 1884. His father was Thomas Gelston Atkins, M.D., eminent both as a physician and a surgeon, and his mother was a daughter of the Very Reverend George Mignon Innes, D.D., Dean of St Paul's, London, Ontario. She died when he was fourteen, which must have been a terrible grief to him, as he was devoted to her, and for many years after her death could not bring himself to mention her name. She played a great part in moulding his character, but he also adored his father, and it is probable that we may trace to their intimacy his leaning towards physiological and even medical problems. His younger brother, now Brigadier R. Gelston Atkins, evidently came under the same influence, and, having taken up medicine as a career, served for many years in the R.A.M.C.

'Billie' Atkins went to the Friends' School, Newtown, Waterford, and to the Grammar School, Cork, and entered Trinity College, Dublin, in 1902. He entered the Medical School, but before very long decided to concentrate on the study of 'Experimental Science', (i.e. physics and chemistry) and 'Natural Science' (i.e. botany, zoology and geology). This he did to such good effect that in 1906, after a number of minor successes, he obtained Senior Moderatorships, i.e. First Class Honour Degrees, in both these subjects, thus laying the foundation of his extremely versatile career as a scientific worker. His early medical training also stood him in good stead later, and especially during the Second World War, when he did useful physiological work for the R.A.M.C. Dublin University later conferred on him the degrees M.A. (*Stip. cond.*) in 1909, Sc.B. (*Stip. cond.*) in 1912, and Sc.D. (*Stip. cond.*) in 1914, the latter degrees being awarded in recognition of the value of his published work. In 1914 he was elected a Fellow of what is now the Royal Institute of Chemistry.

Shortly after obtaining his B.A. degree in 1906 Atkins was appointed as an assistant in the chemical laboratory under Dr Sidney Young, University Professor of Chemistry. This involved much teaching of practical classes, but also left time for research—especially in vacation periods, during a considerable part of which he continued to live in his rooms in College. Young was well known for his work in physical chemistry, and E. A. Werner, the senior lecturer in the department, who eventually, on Young's retirement, became Professor of Chemistry, had done much good work in organic chemistry, in which subject he was a brilliant lecturer.

It was natural, therefore, that Atkins's early papers described work in both these branches of science, some of his work being in co-operation with Werner. His father's medical influence was, however, well to the fore, and Atkins's first published papers were on the freezing points and other properties of biological fluids, most of the results being of immediate importance to his father in the course of his surgical work. This use of the Beckmann freezing point apparatus led to many later determinations of osmotic pressure by cryoscopic methods. He also worked on problems of chemical dynamics and on the fractional distillation of binary and ternary mixtures of liquids—experience which proved invaluable when during the First World War similar problems arose in connexion with aeroplane fuels and varnishes. Much of this work was done in co-operation with other workers.

In 1911 his great friend H. H. Dixon, Professor of Botany, with whom he had already co-operated, was in need of an assistant, so Atkins was appointed to this position, which, however, did not prevent him from continuing his research work in the chemical laboratory. Dixon and Atkins carried out important work on the osmotic pressure of plant juices by the freezing point method, using Dixon's thermoelectric method. Later they added measurements of electrical conductivity of the saps. Observations of the colour changes in expressed saps led Atkins to undertake work on oxidases and their inhibitors in plant tissues.

Much of this work was summarized in a book *Researches in plant physiology*, which he published in 1916 with a view to interesting senior students in this rapidly advancing section of botanical science.

This first period of Atkins's scientific work was brought to an end by the First World War. It had been a very busy and, I think, a very happy time, during which he had spent the greater part of each year living in his rooms in Trinity, which he had come to regard as his second home, since vacation periods gave the greatest opportunities for research work. His work did not prevent him from taking an active part in college life, and he made countless friends. A number of them, like so many Trinity students, fell during the war, but he had a wonderful gift for keeping in touch with many of those who survived, wherever they might be throughout the world.

The greatest of his Trinity friends was his professor, H. H. Dixon, as can be judged by the Obituary Notice which he wrote in 1954. On the cover of the copy which he gave to me is written 'The tribute of disciples'. Dixon, in turn, was a life-long friend of John Joly, Professor of Geology, who always came over to the botanical laboratory for a very simple luncheon. Joly, accordingly, also became one that Atkins looked up to with affection and respect. During summer vacations Atkins often visited Professor and Mrs Dixon at their house in Dooks, County Kerry. Joly was generally a member of the party, living in a tent, or later in a wooden bungalow near by. Thence they used to explore on bicycles the more accessible parts of that beautiful, but hilly, county.

Having spent many of his summer holiday hours on the water in Cork Harbour he was naturally interested in boats, and on entering College he joined the Boat Club and took up rowing, but as the journey from his rooms to the Boat Club course was of several miles through heavy traffic, he soon found that he could not afford the time required for training. He remained, however, a keen member of the club, always ready to give his help. He also played hockey.

Years later, when he was working very hard, he decided to take up boxing, as being the easiest method of obtaining the exercise which he required in the shortest possible time. He accordingly joined the college Boxing Club, rather to the surprise of his friends, as most of his actions were rather deliberate. He could evidently react quickly, however, when necessary, and they were even more surprised when, aided by a long reach, he won the amateur middle-weight championship of Ireland in April 1914.

In 1915 he became Assistant Chemist at Woolwich Arsenal, and next year he went to the National Physical Laboratory as Volunteer Assistant in the Division for Aeronautical Chemistry. This led to a Commission in the Royal Flying Corps as an Equipment Officer, a position entailing the organization and equipment of a laboratory at Aboukir. Here he did much useful work on problems connected with balloon fabrics, aeroplane dopes and varnishes, fuels, lubricating oils, etc. He was also responsible for meteorological measurements.

His most valuable contribution may possibly have been his demonstration of the value of colloidal graphite as an addition to the castor oil used for lubricating the Gnome rotary engines with which many planes were fitted. These revolving cylinder engines were very wasteful of oil, and if one ran out of lubricant before getting back to base the plane usually crashed. Atkins showed that the addition of graphite enabled the engines to run considerably longer, and thus saved many planes and pilots' lives. He was twice mentioned in despatches, and rose to the rank of Major. On his retirement at the end of the war he was honoured by being made an Officer of the British Empire (Military).

An amusing tale is told of an incident on the voyage to Egypt in 1916. He was asked whether he could take any part in some sports which were being organized on board. He replied that he had done a bit of boxing. This caused some surprise, and he was warned that if he entered he might meet a chief petty officer who was the middle-weight champion of the Navy. Atkins replied 'Well, he can only knock me out'. After the competition, in which the chief petty officer was duly floored, Atkins was not allowed to box any more. They said that he spoiled sport, as there was no one on board in his class! As Atkins did not actually deny this story when I taxed him with it years afterwards, I think that it is probably true.

On a later voyage he was less fortunate, as the ship in which he was travelling was torpedoed and sunk with loss of life, he being saved by swimming away from the sinking ship.

When he returned to Trinity in 1919 he went back to the School of Botany. He had become interested in problems connected with the hydrogen ion concentration of soils, waters, and vegetable fluids, and its effect on plant growth, but before he had time to do much work on the subject he was, in 1920, appointed Indigo Research Botanist, Imperial Department of Agriculture, India. Here, besides other work on indigo, he made pH measurements of some Indian soils and plant juices.

He returned from India in 1921, and was appointed Head of the Department of General Physiology in the Marine Biological Association's Laboratory at Plymouth, a position which he retained until his retirement under the age limit in 1955, apart from a period of war work during the later years of the Second World War.

The work of the Plymouth Laboratory is largely concerned with problems having an important bearing on the fishing industry, and as the existence and growth of fish depend on the growth of the phytoplankton on which they feed—directly or indirectly—Atkins at once commenced a study of the marine conditions favourable to plant life and growth. He began by a study of the effects of changes in the hydrogen ion concentration of sea water, and published several papers on the subject.

He had not, however, lost his interest in inland waters and land plants, and in any spare time that might be available he used to explore the countryside, armed with a set of pH colour indicators and a botanical collecting case; in this way he gathered knowledge which he also published.

He then turned his attentions to the phosphate-content of sea water and also of the land waters, using a colorimetric method adapted from one devised by Denigès. He confirmed the observations made by Matthews in 1916 that a large fall in phosphate occurs in sea water in the early summer owing to the outburst of growth of the phytoplankton caused by the bright spring daylight.

He studied this effect over many years, showing that the date of the phosphate minimum depended on the brightness of the spring light. He also found similar changes in the content of silicate which was used up by the growth of diatoms. His results were consistent with H. W. Harvey's nitrate measurements, and in 1925 they published a joint paper on the variation with depth of salts utilized in plant growth. This variation, together with that of temperature, gave information regarding vertical mixing of water columns.

It is difficult for one not in close touch with the development of this important subject to assess the value of Atkins's contributions to it, and I am deeply indebted to Dr H. W. Harvey for the following appraisal of this very important part of his work.

'Then and now an outstanding problem in marine biology was to account for fluctuations in various fisheries which occurred from year to year and over longer periods. Attention was being directed to the supply of phytoplankton, the primary food of the animals, and to the natural changing conditions which affected and governed the growth of these plants. A research by Dr E. J. Allen, F.R.S., then Director of the Plymouth Laboratory, had contributed to this line of enquiry.

'On arrival, Atkins started to investigate the annual changes of several microconstituents in the waters of Plymouth which are related to the growth of phytoplankton, publishing a series of papers on these subjects during the next ten years. These pioneer investigations each led at once to similar and further observations in many parts of the world, and to what is now a very extensive literature.

'He adapted a method due to Denigès of estimating phosphate at great dilutions, and followed its seasonal variation in the English Channel and its variation with depth at a position in the Bay of Biscay. This confirmed an analysis by the late D. J. Matthews indicating that in the sea off Plymouth the plants abstracted all but a minute fraction of the phosphate in solution during the summer. Atkins's method was rapid, and could be used at sea, and soon resulted in a wealth of observations extending from the Barents Sea to the Antarctic. It is, in essentials, the method in use today. It then necessitated the visual comparison of intensity of extremely pale colours, for which he possessed an unusually high power of discrimination.

'While this work was in progress he was also engaged in following the annual changes in pH in the waters of the Channel, making colorimetric comparisons, and measuring volumes accurately with pipettes, which often required almost acrobatic skill in the small rolling and pitching vessel. From this study in distribution he found the decrease in carbon dioxide of the water between winter and summer brought about by photosynthesis. This and the decrease in dissolved phosphate during the same period allowed him to estimate roughly the quantity of plant tissue produced in excess of that remineralized. It is of interest that his first guess-estimates of annual production by photosynthesis below unit area are well within the order which may be deduced from subsequent investigations in similar temperate areas. During this work he noticed that when sea water was kept for a few days in a small container there was a marked decrease in pH due to carbon dioxide set free by respiration of bacteria attacking dissolved organic matter, and in this way estimated the "respirable organic matter" in the water at different positions and seasons. He expanded this inquiry by estimating the organic matter which could be oxidized with alkaline permanganate. The estimation of dissolved organic matter was thereafter pursued by the late Professor August Krogh, For.Mem. R.S.

'Other microconstituents whose distribution were investigated included silicate, which decreased between March and June while diatoms were increasing most rapidly; nitrite, which he found to accumulate in the water during late summer when its further oxidation to nitrate proceeded less rapidly than its formation from ammonium excreted by animals; and copper, which was found to undergo a seasonal variation, decreasing with the onset of rapid phytoplankton growth in the spring.

'A paper was published on the stratification of water in the Channel which takes place from July to September, and the large amount of energy required to break down the thermocline. He was the first, or among the first, to recognize the effect of rough weather or turbulence in carrying phytoplankton organisms down below the depth where growth was possible, and so delaying the onset of rapid increase of the plants in the spring.

'In 1926 his first of a long series of papers on the intensity of illumination in the English Channel was published. This work, using very primitive apparatus initially, was soon followed and extended into other sea areas by other workers. In this country it led to the now classical experiments by Dr P. Jenkins on the rate of photosynthesis of cultures of diatoms immersed to different depths over varying periods of time in the Channel while measurements were made of the light of several wave bands reaching the bottles of culture. These showed that with an average flux of light energy between $0 \cdot 003$ and $0 \cdot 002$ g cal cm^{-2} min^{-1} reaching the plants during 24 hour periods, photosynthesis was proportional to light energy, while below this value respiration exceeded photosynthesis. Atkins's research on the scattering of light by particles in suspension led to direct estimations showing some 6 g dry weight of both organic and inorganic particles per cubic metre in the blue-green waters of the Channel, and about half these quantities in the blue waters of the open ocean beyond the continental shelf.'

Atkins also did a lot of work on the preservation of ropes, nets, and fabrics used under water, and published many papers on this important practical subject.

The value of his work led in 1925 to his election to Fellowship of the Royal Society. In 1928 he was awarded the Royal Dublin Society's Boyle Medal, and a few years later he became a Fellow of the Institute of Physics.

In 1922 he married Ingaborg Jackson, daughter of the late George Miller, J.P., and went to live in an old-world house, the Old Vicarage, near the village of Antony in Cornwall. This was his home for the rest of his life, in which many of his friends have enjoyed the delightful hospitality of himself and Mrs Atkins, with which no shortage of domestic staff was allowed to interfere.

They had one son, George Mignon Gelston, who is now a Captain in the Royal Artillery, and has seen much active service in Palestine and Korea.

The chief drawback of Antony as a place of residence for one working in Plymouth was the inconvenience of transport. His day generally began with a three mile, rather hilly, bicycle ride to Torpoint Ferry, after crossing which two walks and a tram ride eventually took him to his laboratory, the process being repeated in reverse order in the evening. Bus services became available later from Antony to Torpoint, and from the Devonport side of the ferry to near the laboratory, but even then the journey was somewhat time-consuming, especially as the various services rarely synchronized. This inconvenience, however, brings with it one great benefit. Antony is still a pleasant old-world village set in delightful country, instead of being a suburb of Devonport.

Some years later he bought Derry House in the seaside village of Downderry, but he and Mrs Atkins continued to spend the greater part of the year at Antony, her mother, Mrs Miller and her sister living in Derry House until the latter's marriage and, some years later, Mrs Miller's death.

He and Mrs Atkins used to spend part of the year at Derry House, and since it is roomy, they were able to put up their guests also. It is a delightful place to spend a holiday, with a path down the cliff from the garden to the beach, but it was not so convenient for working in Plymouth, as even when an early bus was available it was a long bus ride to Torpoint ferry, and when they stayed there in the late autumn his day's work began with a hilly four-mile walk to catch a morning bus at Crafthole.

Dr Harvey has related how Atkins became interested in the measurement of submarine illumination. He was impressed with the importance of Shelford and Gail's work in Puget Sound, published in 1922, and shortly afterwards wrote to me suggesting that we should collaborate in making similar measurements in the waters off Plymouth. I have always regarded this letter as marking a red letter day in my life, as not only did it bring me the opportunity to play some part in this section of his work, but it led to many years of happy collaboration, which were enriched by a friendship extending far beyond the bounds of scientific research. I have the happiest memories of many pleasant visits to Antony or Downderry, and of the more than kind reception always accorded to me by everyone in the M.B.A.'s Laboratory or on board their trawler *Salpa*, or, on the occasion of my last visit, on their fine new ship *Sarsia*.

I only remember one rather unpleasant day when the *Salpa's* movements were too much for my comfort, though my distress was lightened by the fact that we succeeded in getting our light measurements made. Shelford and Gail had worked in water so smooth that they had been able to use a sensitive galvanometer in a small vessel to measure the current passing through their photoelectric cells. It was obvious that we could not do this, except perhaps on special occasions. In one of his early letters to me Billie wrote 'On the *Salpa's* latest cruise the skipper was thrown out of his bunk.' We accordingly employed a telephone method for measuring the photoelectric currents, reading to about 10^{-9} ampere. He made almost all the measurements, and was expert at effecting a balance, even among the various sounds of a small ship at sea. His experience with a telephone in conductivity measurements probably helped him.

As usual he adapted his marine work to measurements on shore, and made many measurements of the daylight factor in various shaded sites and its correlation with the vegetation. For this work galvanometers could be used. He installed a Burt vacuum sodium cell on the laboratory roof, connected to a recorder in the laboratory, so that permanent records of the hourly and daily variations in the daylight were kept over many years. The permanence of the Burt cell rendered it admirable for this purpose, though unfortunately it is only sensitive to blue light and some near ultra-violet.

Even the potassium cells used in the early work at sea had little green and almost no red sensitivity, so that the development of the thin-film caesium vacuum cell by Campbell about 1930 was very helpful. This being sensitive throughout and beyond the visible spectrum, it could be used with suitable colour filters to measure the penetration of different wavelengths into the sea. Measurements with such a cell without a filter would obviously give an erroneous idea of the comparative opacities of different water layers owing to the rapid extinction of red light. With the potassium cell, sensitive only to blue light, this effect is not very important.

The development of the selenium 'rectifier' or 'blocking layer' cell a few years later greatly reduced the possibility of surface leakage errors, always troublesome in damp weather, and yielded much larger currents without any high-voltage supply. It also made it possible to mount six cells in a compact cubical photometer case, and thus study the angular distribution of the submarine daylight. Most if not all of the rectifier cells which we used suffered, however, from one serious drawback. In the course of some months, or at best a very few years, their sensitivity to bright light declined greatly. The fact that their sensitivity to weak light did not fall nearly so much rendered the defect more serious, as the fact that the response to a standard test lamp had not declined much was no evidence that the sensitivity to strong light was also reasonably constant. The very large range of illumination that has to be measured in marine work renders this a very important source of error, and Atkins finally became convinced that the use of rectifier cells was an unreliable method in routine work unless very frequent standardizations were made in strong light. With the great advance in our knowledge of semiconductors in recent years it is quite possible that this error may not occur with modern rectifier cells. If so they would be ideal, as their sensitivity curve extends throughout the visible spectrum.

On the outbreak of the Second World War he joined the Home Guard, and was put in charge of the contingent from the district round Antony. Their duty was to endeavour to delay any enemy advance along the roads between the Lynher and the sea leading from the west to Torpoint and the west side of Plymouth Sound. He told me years afterwards that the feeling that they were only playing at being soldiers accorded badly with the knowledge that if the enemy did succeed in landing to the west they would almost certainly be wiped out. His period of Home Guard service included the worst of the Blitz on Plymouth, and from the high land round Antony they had a full view of that dreadful sight. The Marine Biological Laboratory was hit and, besides other damage, the Director's House at the east end was burnt.

In 1942 he was given the rank of Captain in the R.A.M.C., and for some time did much routine physiological testing at medical centres throughout the country, resulting in several papers on the vitamin C reserves of troops, and kindred matters.

From 1943 to 1945 he was engaged in work on atmospheric visibility under different weather conditions for the Meteorological Office, Air Ministry.

In this war also the value of his work received official recognition, as in 1951 he was honoured by being made a Commander of the British Empire. This, being a civil award, in no way affected his holding of the O.B.E. (Military) won in the First World War, which he valued very highly.

On his return to Plymouth at the conclusion of the war he at once commenced to pick up the threads of his pre-war work. This naturally was hindered by post-war conditions, but without undue delay he was able to continue and extend his work on daylight measurement, and on variations in phosphate content, and the preservation of nets and ropes.

The measurements with the cube photometer, both before and after the war, had emphasized the importance of scattering in the extinction of submarine daylight, and had appeared to show that most of the scattered light was only deflected through angles less than 90°. To examine this point he set up an apparatus in which a narrow beam of approximately parallel light was passed through a flask which could be filled with any type of water which it was desired to test. The light scattered through different angles was measured by means of a photomultiplier cell, and the results showed that the greater part of the scattering occurred through quite moderate angles, which appeared to show that it was chiefly due to refraction through transparent mineral particles.

He also employed a method for observing the seasonal changes in the phytoplankton by spectrophotometric estimation of the chlorophyll content of a sample of sea water.

These and other developments of his previous work kept him busily engaged until his retirement, under the age limit, in 1955, when he was over seventy years of age, as the age limit had been specially extended by five years in his case.

He then turned his attention to the task of working up the results of several lines of work on which he had been engaged.

He also turned some of his energies to his garden, and here it may perhaps be feared that he overtaxed his physical strength, as he developed phlebitis in one leg. This entailed a long rest in bed, after which he suffered from a severe attack of pneumonia. When he recovered his physical strength was much impaired, and he was afflicted with arthritis, which considerably reduced his powers of walking, and rendered writing somewhat difficult and painful.

His mental powers were, however, as great as ever, and three years later, in 1958, he received one of the big honours of his life, being asked to act as President of the Botanical Section of the British Association Meeting at York in 1959. He at once commenced the preparation of his Presidential Address, choosing as his subject 'Plants on Land and in the Oceans', and carried out a lot of other business connected with the meeting, at which, alas, he was not destined to be present. His Address was barely finished when he was attacked by his last illness, which started with influenza in December 1958. Other troubles developed, eventually necessitating an operation, which left him very weak, and he died on 4 April 1959.

His Presidential Address was read at the British Association Meeting on 3 September, and its publication in the Report of the Meeting forms a fitting climax to his long list of papers.

As will be seen from the above, and from the list of his published papers, his work covered an astonishingly wide range. In these days of specialization few can hope to achieve distinction in more than one branch of science, but Atkins had done useful work in botany, chemistry, and physics, to list the subjects in the order in which they probably appealed to him.

There was, however, one thread which ran through the greater part of his work—the application of physical and chemical methods to the solution of biological problems, and especially to problems concerned with the life and growth of plants.

The remainder of his work was largely concerned with practical problems, or routine problems of urgent practical importance, such as his work for the Services during two World Wars, and his long series of investigations on the preservation of ropes and nets.

His interests and activities covered many things outside his scientific work. He was keenly interested in literature, especially in poetry, and could repeat by heart long sections of his favourite works. Like many Trinity men of his generation he had at least a fair knowledge of both Latin and Greek, and had a keen ear for the sound of words, which he was rather fond of employing in a punning sense.

He disapproved strongly, however, of what he considered to be misuse of words. Thus he wrote to the Royal Society in November 1957 endorsing Professor Blackman's objection to the physicists' use of the word 'plasma' (*J. Exp. Bot.* 1956, **7**, 9-17). Atkins complained that it was a 'barbarous' misuse of a Greek root, being derived from the word πλασμα, something moulded or formed, and that the biologists already used the term in quite a different sense (which physicists think equally barbarous!). Of course nothing can now be done about the matter, as the word has long since passed into the language of physics, but the incident is typical of his independent attitude in many matters.

Above all else, however, he was chiefly interested in people and their activities, though he never stooped to unkind gossip about anyone. He might, occasionally, express disapproval of someone's actions, but generally only spoke of the activities of the many friends whom he regarded with kindly esteem.

He had a keen sense of humour and a fund of amusing stories, some of them against himself, which he could recount in a highly diverting way, so, with his wide range of interests, he was always a delightful companion.

One of his favourite stories concerned the 'unbreakable' glass flask which he obtained one day when he was working in the chemical laboratory in Trinity College. Being troubled with the fragility of the ordinary litre flask he purchased this special flask which was advertised as 'unbreakable'. He made several satisfactory tests, such as hitting it with a hammer and dropping it on the floor, so when his Professor came into the room, he said, 'Look, Professor', and repeated the dropping test. The flask promptly disintegrated into small pieces, and all he could say was 'It's broken!' Professor Young looked at him in horror, wondering, perhaps, whether he had suddenly gone off his head!

The most amusing aspect of this tale is that it records a most extreme form of maltreatment of scientific apparatus—a crime which, in later years at least, he regarded as almost unpardonable, and which he did his best to discourage. I remember the disgust with which he told me of the evil behaviour of a marine zoologist whom he found weighing a piece of dead fish in the unprotected pan of a chemical balance! I think that he classed this worker as rather lower than others who used to smoke (which he never did) and scatter cigarette ash about in the laboratory in which he was making estimations of phosphate in sea water, where its concentration was measured in parts per thousand million.

As already mentioned, he always did his best to keep in touch with his old friends, either by correspondence or, if at all possible, by actual meeting, often under his own hospitable roof, and hence was always a mine of information as to their doings, in which he took such an interest.

He spent little on himself. He did not smoke, and never touched any alcoholic drink, and when in 1946 he bought a small car it was not for himself, but for his son George, as both he and Mrs Atkins were terrified of some serious accident happening with the ancient Austin 7 that George was then driving. Later he decided to learn to drive himself in order that, when the car was available, he might be able to take Mrs Atkins over to Downderry—a journey which she had to make rather often to see her ageing mother and invalid sister, and which was very difficult in summer when the buses were crowded. He duly passed his driving tests, and later when George bought another, he took the car over for his own use.

The action was typical of him. He never enjoyed motoring, but desired to be able to take Mrs Atkins about, and also any carless friends who might be staying with them. He was in fact generous in the extreme, and many cases of his kind and thoughtful actions could be quoted. Thus F. W. Warren, who started as his laboratory attendant and later played a valuable part in some of his work, writes:

'I do indeed think that it would be grand to give Dr Atkins that colourful character that we knew and loved so well. I think that an important part of his character was the way in which he took the keenest interest in the welfare of his subordinates, particularly the very junior members. He was always concerned when they were ill, and I remember the concern he had for me when I had to spend some time in a sanatorium; this was a part of his character which I feel cannot be too strongly stressed. I remember his ways of doing a kindness, he would bring along a dozen or so eggs in an old cardboard box and insist that the box was extremely valuable and that it should be returned to him immediately that it was empty, his only thought being to ensure that one had a steady supply of eggs until one was better again.'

I have learnt from friends that, although for many years he had never been able to spend more than a few weeks in each year in Ireland—and often not that—he continued to pay the subscription which his father had always paid to his parish church in Cork, and also contributed liberally to the general funds of the Church of Ireland.

His godson, Dr R. W. Harkness, who was the son of his first cousin, gives further examples of his generosity, and touches on other aspects of his highly independent outlook, in the following note:

'He was a very kind person and very generous, e.g. he insisted upon contributing to the cost of my education after my father died. This also illustrated the point that he was outstandingly conscientious about all family matters. In fact he formed the only link between a large collection of cousins of various sorts. He used to keep touch by writing and also going to see people. There are many examples of his acting as organizer and helper to members of the family. Sometimes his kindness was embarrassing, as when he insisted on sending me a set of R.A.M.C. brass buttons for my army greatcoat, the idea being that I could buy the coat without the buttons. He would go to endless trouble, and I remember in war time when I was going to Crookham he sent me a timetable of the trains copied out himself and also insisted on writing a large number of letters and introductions to all sorts of people. His sense of family responsibility also led him to tell you off, if he thought you had done something wrong, and he expressed himself directly and forcibly. One did not like him any the less for this, because it was done entirely honestly and without malice. In any case there was generally a good deal of substance in his protests.

'Many letters contained oblique remonstrances, e.g. "Thank you for yours of the 15th, post mark 25th." His protesting spirit also extended outside the family. There were furious notices in the lab on almost every piece of equipment remonstrating against misuse. A good example of this spirit is in a letter in *Nature* on "Presentation of scientific data" (1945, *Nature, Lond.* **155,** 361). I have a number of others in letters, e.g. ". . . It is a damnable book, gives moisture in grams per cu. ft. of air." Many letters contain asides of protest, e.g. against medicals whom he tended to mistrust as unscientific. "I am sorry to hear you have been laid up. That complaint has a way of recurring and is often due to inadequate consumption of liquid—not fluids, *pace* medical customs, that includes gases also." He used to get very annoyed when he knew he was right and no one would listen. "I am trying to stir up folks to use the methods I found so good for preserving ropes. They would not use them for sandbags and look at the millions lost." In the last war he tried hard to get a job in the services similar to the one he had in the first war and was annoyed when he failed to do so. These protests of his illustrate the characteristic of doing what he thought right in the most direct way and apparently regardless of the consequences. The virtue of this habit was not always appreciated by others. He was not easily deterred, e.g. would carry on making light measurements for 4 to 6 hours on end in the cramped space of the cabin of the *Salpa* while throughout the time a visitor to whom he was demonstrating was being sick.* On a number of occasions I have gathered from members of the family, when he could get no other means of transport when coming to visit them he had been known to walk up to 12 miles carrying his suitcase. This persistence, almost inflexibility, was one of the things which made him such a character.'

Thus to those who knew him best it was his character as a man that appealed most. He made no great parade of the Christian faith which he held so firmly, but he made it the guiding principle which he always endeavoured to follow in his treatment of his fellows.

This did not make him one of those easy-going people who always choose the 'line of least resistance'. As Dr Harkness has pointed out, he was quite the reverse. He never interfered unnecessarily in other people's affairs, but if he felt that he had any responsibility in any matter he would adopt the course of action which he considered right, quite regardless of the risk of making enemies—possibly powerful enemies. He was intolerant of injustice, especially an injustice inflicted by a government or other official authority on some defenceless individual, and was quite ready to take up the cudgels in his defence—sometimes with success. Other matters in which he was ready to stress his point of view were generally concerned with important matters of scientific or technical policy—such as his endeavours to induce the authorities to use the methods which he had evolved for the preservation of sandbags.

Dr Harkness has summed up this side of his character as follows:

'Though W.R.G.A. was unorthodox, his views were logical even though the logic was unusual, and he had more insight into the way the world worked than might appear from the difficulty he had in making it see eye to eye with himself. He says about himself in a letter: "Now this isn't boasting—I know a little (or more) of a great many branches of knowledge. That is at once my curse and my forte." He knew many people thought him odd. I have a letter advising "for the life of you don't mention my name as I fear it stinks there"; but I don't think he ever took much account of what other people would think of him in deciding his actions. If he had lived three centuries earlier he would probably have been burnt.'

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* This of course was strictly in accordance with the rule that the valuable time of the ship must not be wasted and a day's work lost merely because a visitor on board was sea-sick.

It might, perhaps, be expected that this independence of spirit might have made him a difficult man to work with, but nothing could have been further from the truth. He was, in fact, an ideal colleague, who suggested almost all the problems which we tackled together, and did far the greater part of the experimental work. In our thirty-five years of happy collaboration he and I had many differences of opinion on matters connected with the physical aspects of our work (I would never have presumed to question his views on the biological or chemical problems involved) but never a single one which had the slightest possible effect on our friendship. Submarine light measurements made in a comparatively small vessel, probably rolling considerably and drifting appreciably, are liable to certain corrections, and it was often my share of the work to evaluate the results of measurements which he had made. I always knew that I could be even more dogged than he was over some particular problem, without imperilling our good relations. He was far too big a man to take umbrage at differences of opinion on such matters.

In spite of his feeling that his independent spirit had made some enemies I think it probable that few who really knew him well will question the verdict of his closest friends 'We shall not look upon his like again'.

My sincere thanks are due to a number of friends who have helped me in my attempt to write a memoir not entirely unworthy of such a versatile and colourful character as W. R. G. Atkins. In addition to Dr Harvey, Mr Warren and Dr Harkness, whom I have quoted above, I would like to thank particularly Mrs Atkins, who has supplied me with some important information, Professor A. V. Hill, who has been most helpful in the collection of material, Mr F. J. E. Hurst of the Library of Trinity College, Dublin, and Mr D. J. Clarke, Mr A. R. Eager and Miss E. Searson of the Royal Dublin Society's Library, all of whom have rendered valuable assistance.

HORACE H. POOLE

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G. M. Bennett.

GEORGE MACDONALD BENNETT

1892-1959

George Macdonald Bennett was born in the City of Lincoln on 25 October 1892, and was the third of a family of two sons and one daughter. His father, the Rev. John Ebenezer Bennett, B.A. (Dublin), after teaching as an assistant master at a private school at Tring, Hertfordshire, started a school of his own at Peckham Rye and subsequently became a Baptist Minister, first at the Thomas Cooper Memorial Chapel, Lincoln, and later at the Mare Street Baptist Chapel, Hackney. His mother, Hannah Martha Grange, who died in 1932, was the second daughter of William Grange, a farmer of Wigginton, Tring, Hertfordshire. Bennett was named after a friend of his father, George Macdonald, the Scottish poet and novelist. In 1918 he married Doris Laycock, who was the only daughter and eldest child of James Laycock, M.P.S., of Fulham, and who had just taken Part II of the Classical Tripos at Cambridge.

When Bennett was nearly one year old his father left Lincoln for Hackney, where his broadmindedness and advanced views led to conflict with some of the senior members of his congregation. In 1899 he collapsed in the pulpit and, although he slowly recovered from the stroke, he had to resign his ministry and was never fully restored to his normal vitality. He opened a private boarding school at Clacton-on-Sea, Essex, where he was soon joined by Harold Picton, B.Sc., who took charge of the school when he died in 1906; Bennett's mother remained as hostess and matron of the school. This school was remarkable in many ways and was very advanced in educational matters. There were fifteen to twenty boys, with three resident and several visiting masters. Languages were taught by the direct method, the heuristic method was employed in chemistry, and open-air practical work served as an introduction to trigonometry. There was a complete system of self-government by the boys, with an elected school council determining the general rules. A school garden was cultivated by all the boys and masters and, although games were played, they were not made as important as in most schools, whilst no outside examinations were taken below matriculation level. The school had a pronounced effect on Bennett, who was a pupil there from 1899 to 1909. Harold Picton, the headmaster, taught science; he had been a pupil of Sir William Ramsay at University College London, and jointly with S. E. Linder had published papers (*J. Chem. Soc.*, 1892, **61,** 137, 148; 1895, **67,** 63; 1897, **71,** 568) on colloids. Bennett has recorded that it was Picton's teaching which led him to take up chemistry as a career.

In 1909 Bennett took a London University Exhibition in Arts with chemistry as a subject and entered East London (now Queen Mary) College as an internal student for intermediate courses in pure and applied mathematics, physics and chemistry. In 1910 he was awarded the Drapers' Company Soley Exhibition, in 1911 a University Exhibition in Science and in 1912 a Scholarship in Chemistry. In 1911 he took the B.A. (London External) in French, Latin, physics and chemistry, and in 1912 obtained his B.Sc. (London Internal) with First Class Honours in chemistry. During this period his chemistry teachers included J. T. Hewitt, F.R.S., Clarence Smith and F. G. Pope, and C. H. Lees, F.R.S., was responsible for physics. It was here that Bennett was fired with Hewitt's enthusiasm and began research, collaborating as a research assistant with Hewitt in an attempt to prepare an optically active ether, with A. D. Mitchell on molecular surface energy, and with E. E. Turner on the action of the Grignard reagent on chromic chloride. In 1913 Bennett obtained an Open Exhibition in Mathematics and Natural Sciences at St John's College, Cambridge. He became a Foundation Scholar in 1914 and took First Classes in the Natural Sciences Tripos Part I in 1914 in chemistry, physics and mineralogy, and in Part II in 1915 in chemistry. As an undergraduate he continued original work and examined the surface tension of acetic acid. From 1915 to 1918 Bennett was a research assistant to W. J. (later Sir William) Pope, and during the 1914-1918 war he carried out work on explosives, dyestuffs and war gases. The work with C. S. Gibson on the process adopted for mustard gas led to a sustained interest in sulphur chemistry which formed one of Bennett's main themes of research in subsequent years. It was during this period also that Bennett developed his interest in crystallography; in his spare time he made a crystallographic examination of the alleged isomers of ferrocyanides, and he frequently reverted to crystallographic studies in later years; probably his happiest hours were spent with a goniometer or with a polarizing microscope. In 1918 he gave up the work he was doing on conscientious grounds and took a post as research chemist in charge of the laboratories of Messrs Strange and Graham Ltd, a Quaker firm manufacturing fine chemicals, particularly by fermentation processes; here he was responsible for the establishment of the commercial production of citric acid from cane sugar. But Bennett was not entirely happy with a position in industry, and in 1921 he accepted the senior demonstratorship in the department of chemistry at Guy's Hospital Medical School; in 1924 he was appointed Lecturer in Organic Chemistry at the University of Sheffield, in succession to J. Kenner, who had been elected to the Chair of Chemistry at Sydney. This appointment was the equivalent of a modern senior lectureship, and W. P. Wynne, the First Professor of Chemistry at Sheffield, who was largely absorbed in his duties as Dean of the Faculty of Science and in other administrative work in the University until his retirement in 1931, left Bennett with wide opportunities for research. He had the choice of the postgraduate students who wished to undertake research work in organic chemistry, and under these circumstances Bennett, encouraged by

Wynne in every way, built up a small but enthusiastic research school which made outstanding contributions to the chemistry and the stereochemistry of the disulphoxides and related substances. When Wynne retired in 1931, Bennett was appointed to the Firth Chair in Chemistry, but with no deanly duties, and devoted his time to researches on sulphur compounds, the influence of substituents and geometry on the reactivity of organic molecules, liquid crystals and molecular complexes. His students at Sheffield remember well his great enthusiasm for research, and most of them derived great inspiration from him. He was excitable, full of nervous energy, and was frequently seen running to the library to look up some reference or other. He had no time for 'slackers' and would not tolerate muddled thinking. For him there were no such things as organic, inorganic and physical chemistry—they were all chemistry. He was not only interested in his own research problems, but also in those that others were pursuing in the department; in physical and inorganic equally with organic chemistry. Sheffield, in the days of Bennett, was a financially poor university, and his success in keeping active research alive in a department with a small staff, a very small group of research students and a departmental grant of £500 to £600 per annum was an amazing achievement.

In 1938 Bennett was appointed to the Chair of Organic Chemistry at King's College London, in succession to S. Smiles. A. J. Allmand was Daniell Professor of Chemistry and head of the department, but at the outbreak of war Bennett took charge of the department when it moved to Bristol in 1939, Allmand remaining in London on war work. In Bristol Bennett had most happy relations with W. E. Garner and E. L. Hirst under extremely difficult and overcrowded conditions; many King's College and Bristol classes were pooled for economy in man-power and Bennett and his staff took a very full share of the teaching load. It was here that Bennett developed an interest in nitration which led to the important work carried out with J. C. D. Brand, Gwyn Williams and others on the mechanism of nitration and the nature of the nitrating agent in mixed acids. In 1943 Bennett returned to King's College with the department in spite of the fact that the building had been badly damaged by blast and small fires.

In 1945 Bennett relinquished his Chair at King's College and became Government Chemist in succession to Sir John Fox. This decision came as a big surprise to all his academic friends, particularly as he had previously shown little interest in anything but academic work and had frequently criticized and refused industrial collaboration and co-operation. The appointment coincided with the reorganization of the Scientific Civil Service following the report of the Barlow Committee, and Bennett accepted the post in response to a request from a senior official in the Treasury. It is probable that he did so from a sense of duty rather than from personal choice. As Government Chemist he served on the Interdepartmental Scientific Panel set up by the Treasury on matters concerning the Scientific Civil Service, and he became a member of the Royal Commission on Awards to Inventors set up in 1946. On completion of the Commission's work in 1955 he became a member of the Central Committee on Awards. At the Government Laboratory Bennett encouraged research work in a department which was not primarily a research organization, and he was particularly interested in the development of the new instrumental techniques such as X-ray diffraction, X-ray fluorescent spectroscopy, infra-red and ultra-violet absorption spectroscopy and gas chromatography.

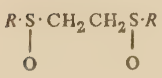
Following a severe heart attack in 1953 Bennett withdrew from most of his outside activities and devoted himself almost exclusively to his official duties. The loss of his wife in December 1957 was a very severe blow to him and he failed to survive a further heart attack on 9 February 1959.

SCIENTIFIC WORK

Bennett had wide scientific interests extending from physicochemical topics such as the goniometric aspects of crystallography, the solubility of ethers in water, liquid crystal studies and the nature of molecular complexes, to his major contributions in (*a*) classical stereochemistry, (*b*) studies on the reactivities of organic molecules, and (*c*) aromatic nitration.

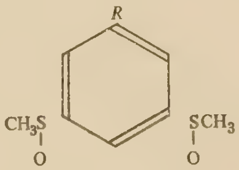
(*a*) *Stereochemistry of sulphur compounds*

The discoveries that sulphinates (1925. H. Phillips, *J. Chem. Soc.* **127,** 2552) and sulphoxides (1926. P. W. B. Harrison, J. Kenyon & H. Phillips, *J. Chem. Soc.*, p. 2019) could be obtained in optically active forms proved the non-planar arrangement of the valencies of tricovalent sulphur quite independently of the still disputed number of electrons in the sulphur-oxygen bond. Consequently disulphoxides of type (I), possessing similar asymmetric sulphur atoms, should occur in the optically inactive forms corresponding to the racemic and meso tartaric acids. Although suitable sulphoxides had been known for many years, such isomerism had not been observed before Bennett and his collaborators, in a series of researches commencing in 1927, discovered numerous examples. For example,



$R\cdot\underset{\overset{|}{O}}{S}\cdot CH_2CH_2\underset{\overset{|}{O}}{S}\cdot R$

(I)



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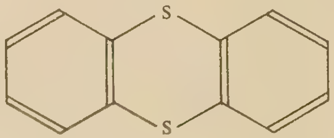

(III)

(I: R=methyl, phenyl, *o*-tolyl, *o*- and *p*-nitrophenyl, *m*-carbomethoxyphenyl, benzyl and $CH_2.CO_2H$) were all isolated in α- and β-modifications, of which the latter were the more soluble in water. Similarly in the aromatic series the compound (II) (R=H), the corresponding *p*-isomer (30)* and the acid (II; $R=CO_2H$) were obtained in α- and β-forms (39). In the case of the acid

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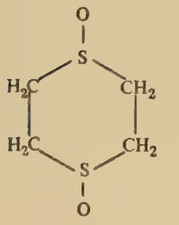
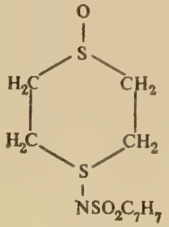
* Numbers in parentheses refer to the numbered bibliography at the end of the memoir.

(II; $R{=}CO_2H$), the more soluble β-modification was partially resolved with brucine, thus proving the racemic nature of the β-oxide; the α-form was unresolvable and consequently the meso-modification. Incidentally, Bennett *et al.* (53, 54) and E. Bergmann & M. Tschudnowsky (1932. *Ber. dtsch chem. Ges.* **65,** 457) independently (1) established the folded structure of thianthren (III), (2) prepared α- and β-modifications of the dioxide and (3) showed the β-form had the trans structure from dipole moment measurements. As thianthren carboxylic acid could not be resolved, it was concluded that the folded structure was flexible and not rigid.

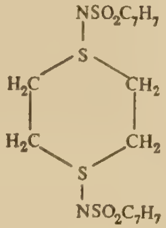
Numerous cyclic disulphoxides and related compounds such as (IV), (V) and (VI) have been isolated in α- and β-modifications (22, 25),



(IV)

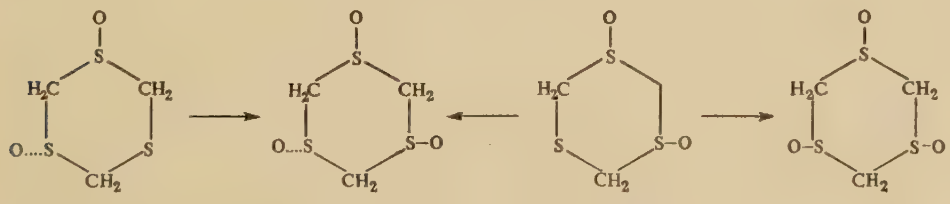


(V)



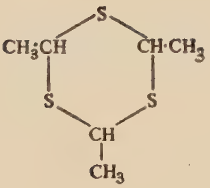
(VI)

and in Bennett's last publication (86) it was shown that oxidation of 1:4-diphenylpiperazine with hydrogen peroxide yielded the isomeric dioxides analogous to the α- and β-forms (IV) from dithian. Particularly interesting results were obtained with trimethylene trisulphide (31), which in accordance with theory gave two di- and two tri-sulphoxides. It was found that the α-disulphoxide gave the α-trioxide only on further oxidation but the β-disulphoxide yielded a mixture of α- and β-trioxides. It follows that the α- and β-dioxides are the trans- (VII) and *cis-* (VIII) forms respectively, and that the α- and β-trioxides have the *trans-cis* (IX) and *cis-cis* (X) structures respectively

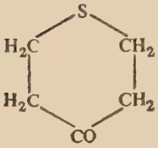


(VII) (IX) (VIII) (X)

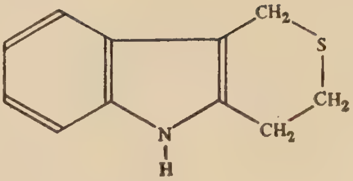
During this work it was shown that trithioacetaldehyde (XI) occurred in two forms in agreement with theory, and a third form reported in the literature was shown to be a eutectic mixture of these two forms (34).



(XI)

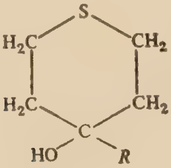


(XII)

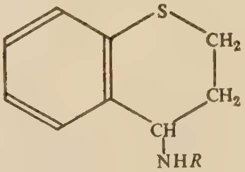


(XIII)

One of the most interesting and novel facets of this work is concerned with pentamethylene sulphide (thiopyran) and its derivatives. Previous approaches to these compounds were wasteful and tedious, but thiopyran-4-one (XII) was readily prepared from ethyl β-thiodipropionate and sodium ethoxide (17). The ketone (XII) was converted (37) by Fischer's method into the tricyclic indole derivative (XIII), and by using the cyanohydrin or Grignard reagent into the compounds (XIV) where $R{=}CO_2H$, C_6H_5 and $C_6H_5.CH_2$, all of which gave isomeric forms of α- and β-sulphoxides (38). In another investigation thiochromanone was converted by reduction of the oxime into



(XIV)

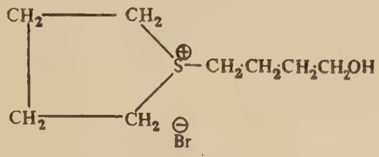


(XV)

4-aminothiochroman (XV; $R{=}H$) which was resolved into optically active forms, and the *d*- and *l*-benzoyl derivatives (XV; $R{=}CO.C_6H_5$) each yielded a pair of stereoisomeric sulphoxides which were separated into optically pure forms (45).

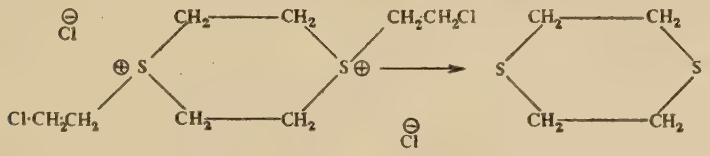
(*b*) *Reactivity of organic molecules*

During investigations arising from the production of mustard gas, Bennett prepared pure specimens of related and relatively simple sulphur compounds (78, 10, 15) and was much impressed by the high reactivity of the hydroxyl groups in monothioethylene glycol, ββ'-dihydroxydiethylsulphide and disulphide, which resembled in many ways that of tertiary alcohols. The β-chloroderivatives also underwent rapid hydrolysis, but the corresponding γ-hydroxy and γ-chloro derivatives were much more stable, and an extension of the enquiry to the δ-series led to an abnormal result (18, 19). When δδ'-dibenzyloxydibutylsulphide was warmed with hydrobromic acid, instead yielding δ-dihydroxy or δ-dibromo-dibutyl sulphide, it gave the cyclic salt (XVI) which yielded tetrahydrothiophene on heating. Although intramolecular salt formation of this type was normally only observed when tetra- and pentamethylene sulphides were produced, intermolecular sulphonium salt formation is frequently observed and several peculiar reactions were explained on this basis (19). For example, the presence of dithian (XVII) in ββ′-dichlorodiethyl sulphide is readily explained by



(XVI)

assuming salt formation and subsequent loss of two molecules of ethylene dichloride. The developments of these researches followed two main lines, (*a*) the formation of many membered polymethylene sulphide rings and (*b*) a study of the factors influencing the reactivity of the chloro- and hydroxyl



(XVII)

groups in the substituted sulphides. These developments necessitated the improvement or the discovery of methods for the preparation of the polymethylene glycols and chlorohydrins as far as the octadecamethylene homologues (33, 46, 68). It was shown that the cyclization reaction

$$CH_3.S(CH_2)_nCl \longrightarrow CH_3.S^+<(CH_2)_n \quad Cl^-$$

occurred smoothly when 'n' =4, 5 and 6, although the rate diminished as 'n' increased (36, 66). However, by using the dilution principle employed in other fields by K. Ziegler (1934, *Liebigs Ann.* **513,** 43), Bennett & H. Gudgeon (71) converted the chlorosulphides where $n=14$, 16 and 18 into the crystalline tetra-, hexa- and octadecamethylene sulphides respectively, of which the first two had pleasant musk-like odours resembling those of the many membered cyclic ketones, lactones and anhydrides. Incidentally, after the completion of Bennett's work in this field, A. Müller & A. F. Schütz (1938, *Ber. dtsch chem. Ges.* **71,** 692) prepared the cyclic monosulphides where $n=12$, 13 and 14, by the action of sodium sulphide on the corresponding polymethylene dibromide. Bennett was unable to prepare cyclic monosulphides in cases where $n=7$, 8, 9, 10 and 12 as high molecular weight polysulphides were produced. These results are in full agreement with observations in the homocyclic and cyclic oxide fields where rings of eight to thirteen members are less easily obtained than larger ones, presumably because of some difficulty in packing the atoms of the chain into a form suitable for cyclization (K. Ziegler, *loc. cit.*; E. W. Spanagel & W. H. Carothers, 1935, *J. Amer. Chem. Soc.* **57,** 929).

During the early sulphur work it was shown that the reactivity of the chloro- and hydroxy-sulphides varied in an unexpected way with the number of methylene groups separating the sulphur and the chlorine or oxygen atoms (19, 21, 36) and further researches placed these qualitative results on a more

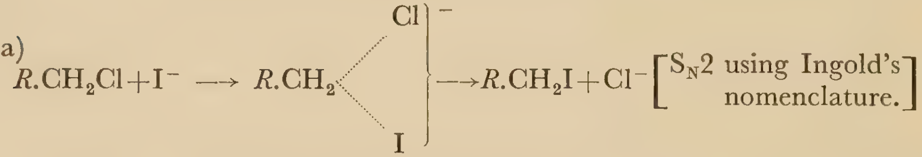
$$R.(CH_2)_nOH \qquad\qquad C_6H_5.S.CH_2.CH_2Cl$$

(XVIII) (XIX)

quantitative basis. The relative rates of conversion of alcohols of types (XVIII; R=S*Ph*) (42, 47), (XVIII; R=OH) and (XVIII; R=O*Ph*) (57) into the corresponding halides showed a marked optimum in all three types when $n=4$, corresponding to the ready cyclization of the halogen sulphides when $n=4$, referred to above. Bennett suggested that the reactivity was due to a transmission of the influence of the sulphur or oxygen atoms to the δ-position by the 'direct effect' postulated by R. Robinson *et al.* (1926, *J. Chem. Soc.* p. 406) in their researches on substitution of ethers of catechol and hydroquinone.

In 1927 (21) it was observed that the reactions of chlorides of type (XIX) with (*a*) an iodide ion and (*b*) a hydroxyl ion were accelerated and retarded respectively by the introduction of a nitro group into the aromatic ring, and it was concluded that the reactions were of different polar types similar to those encountered (1922, A. Lapworth & J. B. Shoesmith, *J. Chem. Soc.* **121,** 1391) during the hydrolysis and reduction of the isomeric methoxybenzyl bromides. Many similar variations in reactivity were examined by other workers around this period, and Bennett & G. Baddeley (49) explained the reactivities of the chlorides (XIX) in terms of different mechanisms for the reactions (*a*) and (*b*). Thus reaction (*a*) was interpreted in the following steps:

(a)



$$R.CH_2Cl + I^- \longrightarrow \left[R.CH_2 \begin{matrix} \cdot\cdot\cdot Cl \\ \cdot\cdot\cdot I \end{matrix} \right]^- \longrightarrow R.CH_2I + Cl^-$$

[S_N2 using Ingold's nomenclature.]

in which the rate determining first stage was facilitated by electron attracting substituents in R. Reaction (*b*) was formulated thus:

(b)
$$R.CH_2Cl \longrightarrow R.CH_2^+ + Cl^-$$
$$R.CH_2^+ + OH^- \longrightarrow R.CH_2OH$$

[S_N1 using Ingold's nomenclature.]

in which the rate determining first stage will be inhibited by electron attracting substituents in R.

A more detailed examination (49) of the influence of substituents on sulphides of type (XIX) showed good overall agreement with the generalizations; and the order, p-O*Me*<p-CH_3<H<p-halogen<p-nitro observed in reaction (*a*) was exactly reversed in reaction (*b*). But several abnormal features were encountered which called for special explanations. Firstly, it was observed that the effect of substituents in an ortho-position was frequently the reverse of their normal influence; thus in studies of reaction (*a*) it was found that an ortho chlorine substituent diminished reactivity but para substitution by chlorine resulted in increased activity. Bennett (50) attempted to connect these observations with a 'reversed field effect' acting directly through space but of opposite sign from that of the usual inductive effect, but the suggestion was criticized and never readily accepted. An even more puzzling observation was made during a comparison of the effects of the halogens. In reaction (*a*) all halogens facilitate reaction and in the order $Cl > Br > I$ as could be expected from the inductive effects of the halogens as deduced from the strength of acids. In reaction (*b*) all halogen atoms inhibit hydrolysis but the observed order of reactivity is $H > Cl > Br > I$ and Bennett concluded that the electromeric and mesomeric effects of the halogens must fall in the order $F > Cl > Br > I$. In order to gain further knowledge of the behaviour of the halogens Bennett, together with Brynmor Jones, G. Baddeley, S. Glasstone and others (58, 59, 60, 61) made accurate measurements of the reactivities of a large number of substituted benzyl and β-phenylethyl chlorides and of the dissociation constants of a series of halogenated anilines and phenols. The systems are probably much too complicated for simple interpretation, many factors are involved and it is concluded that a satisfactory explanation of the facts necessitates consideration of (*a*) polarizability effects varying in the order $I > Br > Cl > F$, (*b*) inductive and direct effects varying in the order $F > Cl > Br > I$, (*c*) mesomeric and electromeric effects also varying in the order $F > Cl > Br > I$.

It may be recalled that Bennett's suggestion that the electromeric and mesomeric effects of the halogens were in the order $F > Cl > Br > I$ was the reverse of that favoured, largely on theoretical grounds, at the time, and the new proposals did not escape criticism. However, the change of viewpoint has since been justified both on a theoretical basis and also by the work of others on dipole moments, nuclear substitution, side chain reactivity, acid strengths, etc., and the order proposed by Bennett is now widely accepted.

(*c*) *Aromatic nitration*

While at Sheffield Bennett had shown interest in the nitration of aromatic sulphonic acids and sulphonic chlorides, in nitration reactions leading to phenolic by-products and in preliminary attempts to examine the nature of nitric acid in oleum solution. It was therefore not surprising that the wartime nitration atmosphere of the Bristol laboratories in which Bennett unexpectedly found himself in 1939, provided favourable opportunities for the development of these interests. Although it was often suggested that the nitronium ion, NO_2^+, was the actual nitrating agent under the usual conditions of aromatic nitration, it was not until 1946 when Bennett and his colleagues and other independent workers in this country, in Europe and in the United States produced convincing proof. Early spectroscopic work of A. Hantzsch (1925, *Ber. dtsch chem. Ges.* **58,** 941) showed that in sulphuric acid solution, nitric acid existed in a form differing from undissociated nitric acid or the nitrate ion. The high electrical conductivity of nitric-sulphuric acid mixtures suggested the formation of ions, and Bennett, with J. C. D. Brand and G. Williams (79) showed that the nitric acid was converted into a cation which migrated towards the cathode during electrolysis, as expected on the basis of equation (*a*). Bennett *et al.* supported this equation by measuring the reduction of the partial pressure of nitric acid in excess sulphuric acid and studying the influence of nitric acid and bisulphates on

$$HNO_3+2H_2SO_4 \rightleftharpoons NO_2^+ +H_3O^+ +2HSO_4^- \qquad \text{(a)}$$

the titratable sulphur trioxide content of oleum (81). These conclusions were confirmed by a reinterpretation (78) of A. Hantzsch's (1908, *Z. phys. Chem.* **65,** 41) earlier cryoscopic results and by the later more precise results of C. K. Ingold *et al.* (1946, *Nature, Lond.* **158,** 480). The ultra-violet and Raman spectroscopic studies of J. Chédin (1946, *Chim. et Ind.* **56,** 7) and others are also consistent with the equation (*a*), and it has been shown that the Raman shift $\Delta\nu$ of 1400 cm^{-1}, characteristic of the nitronium ion, NO_2^+, becomes observable at limiting acid compositions, corresponding closely with the 'threshold' values which J. A. Hetherington & J. I. O. Masson (1933, *J. Chem. Soc.*, p. 105) showed were necessary in the two-phase nitration of nitrobenzene (78). This work identifies the nitronium ion, NO_2^+, as the active nitrating agent of mixed acids.

During other experiments designed to throw light on the manufacture of trinitrotoluene, it was shown that the rate of nitration of 2:4-dinitrotoluene in a two-phase system was proportional to the molar excess of sulphuric acid over water present, but a complete analysis of the results was not possible owing to the lack of data concerning the concentrations in the two phases (81). The general validity of equation (*a*) was supported by accurate measurements of the nitration of 2:4-dinitrotoluene in homogeneous sulphuric acid solution at varying temperatures and using acids of varying strength (80). It was shown that the rate of nitration had a maximum value at 92 per cent sulphuric acid, and Bennett *et al.*, in order to explain the results, postulated the termolecular reaction (*b*), requiring both attachment of the NO_2^+ cation to the benzene nucleus, and also the removal of the proton by the bisulphate ion.

$$R.H+NO_2^+ +HSO_4^- \longrightarrow R.NO_2+H_2SO_4 \qquad \text{(b)}$$

Some observations, such as the unpredicted dependence of the velocity constant on the concentration of the 2:4-dinitrotoluene, have been ascribed by Bennett *et al.* to simultaneous oxidations, but the ternary collision required for the mechanism (*b*) has not been accepted, and E. D. Hughes, C. K. Ingold *et al.* (1950, *J. Chem. Soc.*, p. 2400) have later advocated a two-stage process (*c*).

$$R.H+NO_2^+ \xrightarrow{\text{slow}} R.H.NO_2^+ ;\ RHNO_2^+ +HSO_4^- \xrightarrow{\text{fast}} R.NO_2+H_2SO_4 \qquad \text{(c)}$$

Bennett was awarded the Ph.D.(London) in 1924, and the Sc.D.(Cantab) in 1932. He was a Fellow of St John's College, Cambridge, from 1917 to 1923, and of Queen Mary College, London, from 1939. He was elected to the Fellowship of the Royal Society in 1947 and was created Companion of the Order of the Bath in 1948. Bennett's services to chemistry were numerous. He was an Annual Reporter of the Chemical Society, a member of Council from 1929 to 1932, Honorary Secretary from 1939 to 1946, and Vice-President from 1948 to 1951; he represented the Chemical Society on the Barker Crystal Index Committee from its inception in 1945; he was a member of the Council of the Royal Institute of Chemistry from 1949 to 1951, an examiner from 1944 to 1949 and a Vice-President from 1951 to 1953. He served on the Council of the Faraday Society from 1946 to 1948, as Director of the Bureau of Abstracts from 1946 to 1954, and as Honorary Secretary of the Chemical Council from 1945 to 1951.

He was shy, reserved and difficult to get to know well, but he was generous, courteous, scrupulous and conscientious, with a high sense of responsibility. He was tremendously interested in anyone who was doing good scientific and literary work and he was full of admiration and enthusiasm for the scholar. On the other hand, he could be very critical of anyone who, he felt, was not using to the full his opportunities of research, and of those who seemed to be proceeding in a dull and uninspired way. He was a great reader, and although his choice was at first very wide, it became more concentrated on detective and crime stories as time went on. Reading and walking, with frequent visits to the cinema, were his chief relaxations. His views on most things were based on sound common sense and he detested any forms of show or self-advertisement. He had no children of his own, and after some family and other bequests he left the residue, forming the bulk of his estate, to Dr Barnardo's Homes.

The grateful thanks of the writer are due to a number of G. M. Bennett's colleagues and students for much help in the preparation of this notice.

R. D. Haworth

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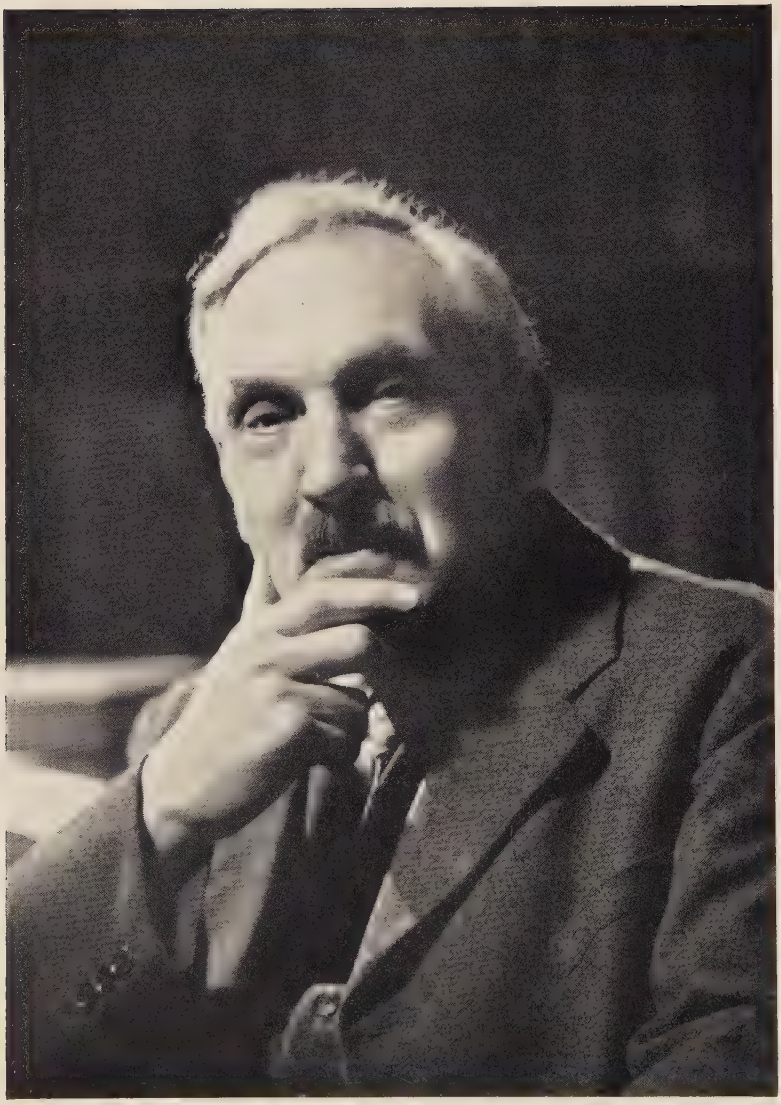

F. J. Cole

FRANCIS JOSEPH COLE

1872-1959

F. J. Cole was the fifth in a family of six children who were born to William Cole and Elizabeth Cole, formerly Elizabeth Patterson (*née* Watts); his mother had had no issue by her first marriage, and Francis was born on 3 February 1872 at Clapham. His paternal grandmother made a fortune in business in the West End of London, and thereby her three sons were left in easy circumstances. But not one of them inherited the prudence and business capacity of their mother, and only the early death of Cole's father when Cole was three saved the family from financial embarrassment, while their mother had to provide for the children on an income derived from wasting securities. Cole's sister, Sophie, was to be the authoress of some fifty novels, and to continue writing until she was over eighty.

His primary school was a small one associated with St Paul's Church, Clapham, where the distinguished Martin Lister (*ca.* 1638 to 1711/2; F.R.S.) was buried. This school had a very capable headmaster, J. R. Crawford, who used the stick vigorously, but thereby knocked more sense into his pupil, Cole, than the latter acquired at his secondary school (Sir Walter St John's, Battersea), where—quite foolishly—the stick was regarded as brutalizing.

In those days the only sciences taught at this latter school were chemistry and physics, but the boys were not allowed to take both, nor was there any specially trained science master. Once a week Cole made a browse round the sixpenny box in Salkeld's bookshop in the Wandsworth Road, and he got to know Mr Salkeld, while one of the first books purchased by the boy, namely Bastian's *The beginnings of life*, was still in Cole's library at his death, nearly seventy years later. At the school his chief outside interest was in cricket, which he played at Mitcham, and he practised bowling daily until he achieved a high percentage of hits on a penny placed on the ground. He was a left-hand bowler. This interest in cricket, incidentally, lasted to the end of his life and, whenever he could spare the time, he would watch first-class matches at the Oval or Lord's, especially when Surrey were playing.

Up to the time at which he left school, the only academic success which Cole had gained had been in the writing of essays. Though he had also won a prize for mathematics, he thought ever afterwards that that might have been a mistake. The success in essay-writing suggested that journalism might be a suitable occupation for the young boy, and he began on a local paper in Clapham, then went on to Fleet Street, and later was on Brighton and East Grinstead papers, and also the *Sussex Daily News*. For a time he even worked on the staff of a furniture paper, though he admitted that he knew nothing about furniture. While working on a London daily paper, he was sent to report the trial of Mr Horatio Bottomley, accused of dealings in bogus printing works. The defendant acted as his own lawyer and was acquitted.

Cole, however, was not happy in newspaper work, and about this time the main evidence was of a desperate striving on his part to educate himself. He took private lessons in chemistry, French and German from a Mr Herbert Robson, whom he always considered one of the most remarkable men he had ever met. Latin he learned from a Mr Jones, and Greek he taught himself because he was aiming for Oxford University entrance, in the examination for which Latin and Greek were compulsory. It is difficult to say exactly when and how he became interested in science, and he himself thought that the school had had less to do with it than had his old schoolfellow, J. O. Borley, who later distinguished himself in fisheries research, though he failed to secure election into the Royal Society. The two of them dissected animals together, acquired an ancient microscope, and devoured *Nature* every week. Their text was Huxley and Martin's *Practical zoology*, and they had been inspired to dissect earthworms 'after reading Darwin's work on that despised annelid'. Cole's Victorian mother did not, strangely enough, object to the dissection of worms and frogs in his bedroom, 'or even to the doubtful odours to which these strange occupations gave rise'. It was only later that Cole discovered Ray Lankester's three astonishing papers on the anatomy of the earthworm, written in 1864 when Lankester was only seventeen years of age. More immediately important was the classical paper on the earthworm's development, which was published in 1889 by 'that quiet cultivated gentleman' Edmund B. Wilson. At the cost of some weeks' pocket money Cole and Borley bought a copy, but they were disappointed to find that they could understand hardly a word of it, though it was—strangely enough—a startling revelation of the possibilities of research. It was not the first book that Cole had bought, but he always regarded it as the first to arouse that bibliographical meticulosity which was to develop into a dominating influence throughout the rest of his long life. He decided about this time that his future lay in zoology, and he attended classes in that subject at the Royal College of Science under Professor G. B. Howes, 'who was the most learned zoologist' Cole ever met. He also attended lectures at the Royal Institution by Mr J. G. Romanes, who recommended him to a friend, Professor J. Cossar Ewart, of Edinburgh University. The latter appointed the young man as his private assistant in 1892; the salary was a modest £1 a week, and Cole had to live on it. Also his knowledge of zoology was still as yet very slight and fragmentary, and he had to work very hard to keep pace with his obligations. At 10 o'clock every night, the chief janitor of the Old University, in his prowl round the building, found Cole at work in the Natural History Department, and promptly drove him forth. In one respect the apprenticeship with Professor Ewart proved specially salutary, since Cole was thereby associated with two very remarkable Orcadian students. These were Sutherland

Simpson, ex-North Sea fisherman, who was later to go to the United States and to become Professor of Physiology in Cornell University; and John Flett, a brilliant man who graduated in letters, science and medicine, and whose opportunity came finally in geology. In due course he was Director of the Geological Survey for Scotland and thereafter of that for England, and was knighted. Though Cole did not graduate at Edinburgh, he produced advanced papers on the nervous system of *Chimaera* and of *Gadus* in 1895 and 1896.

Dr Romanes took a great interest in him and was responsible for his entry into Christ Church, Oxford, where he was to have been his sponsor's research assistant. But Romanes became ill and Cole's funds were insufficient to maintain him and he was obliged to leave Oxford.

In 1894 Cole went as Lecturer in Zoology to University College, Liverpool (afterwards the University of Liverpool), where he stayed twelve years under Professor W. A. Herdman. Realizing that a university degree was essential to his further advancement, and still hoping for an Oxford one, he made contact once again with Dr Romanes and went up to Jesus College to work for a research degree. But he found that he could do so only by first earning his living at Liverpool. Hence it was arranged that he should take his full part in the work of the Department during term times, and go up to Oxford during the Liverpool vacations. In this way he obtained a B.Sc. degree by research, and was one of only three Oxford graduates in that anomalous position.

On 24 November 1898 he married Annie Clow Menzies, of Kirriemuir, Angus, who survives him, and there is one child of the marriage, Frank Menzies, who was educated at Abingdon School and is now a master farmer in Ontario, Canada.

Cole made many friends at Liverpool, chief of whom was James Johnstone, the fisheries expert, with whom he wrote the memoir on *Pleuronectes* in 1901. Both worked late in the laboratory, and, when work was over, played football between the benches and among the bottles. There were great names on the staff in those days. John Sampson the Librarian, Augustus John, E. W. Marchant the electrical engineer, Boys the physicist, Sherrington the physiologist, Mackay the historian, Kuno Meyer the Celtic scholar, and Elton and Raleigh in English literature. Sampson studied the gypsies and their language in Wales; Cole sometimes went with him and the two stayed in the camp, while in their turn gypsies often visited Mr and Mrs Cole in their home at Broad Green.

Cole's interest in marine zoology dated from the Liverpool days. He worked with Professor Herdman and the Liverpool Marine Biological Association members at Puffin Island, off Anglesey; with the Scotts at Piel, Lancashire; and at Port Erin in the Isle of Man, acquiring through these three channels a remarkable knowledge of marine fauna. Later he was to train his students in shore collecting, tow-netting and dredging; and no student who went to Port Erin with him will ever forget his happy companionship in such delightful surroundings, and the enjoyment of working with him on the shore and in the boats.

While at Liverpool he acted as scientific adviser in law cases involving insect damage to foodstuffs. One such case dealt with a cargo of wheat from Australia, which on arrival was found to be so riddled with grain weevils that it was useless. Cole took a sample of grain in a match-box to the court, passed it round, and asked the judge, counsel and jury to state whether the grain was sound. All decided that it was in good condition. He then scraped away the coat of the grain and showed the weevil larva wriggling inside. But counsel was not even then convinced. He asked, 'If a cargo of grain left Australia in good condition, but on arrival in this country was found to be weevilly, how would you explain that?' Cole replied, 'I shouldn't explain it, because it wouldn't be true'. This won the case and he received a substantial honorarium, with which he bought his first grand pianoforte.

In 1901 he had interrupted the tenure of his appointment under Professor Herdman in order to read for the Oxford B.Sc. degree, and it was as an undergraduate of that University that in 1902 he was awarded the valuable Rolleston Memorial Prize, given to Oxford and Cambridge in competition.

In 1906 he left Liverpool to be appointed Lecturer in Zoology at University College, Reading. The department to which he went was a very small one, but Cole as its leader built it up steadily and successfully. There was little in the way of equipment, so he contributed his own collection of perfectly stained slides, and founded the Zoological Museum which is now called by his name.* In 1907 he was made the first occupant of the chair of zoology and he held it until his retirement in 1939. His research at Reading continued with the series of beautiful papers on the morphology of the myxinoid fishes up to 1925, and for the early part of this he was awarded in 1908 the Neill Gold Medal and Prize of the Royal Society of Edinburgh. He was given permission to supplicate for the degree of D.Sc. at Oxford in 1910.

In 1914 he went to Australia with the British Association, and though the meetings were curtailed owing to the outbreak of war, he was able to meet many Australian colleagues and to collect *Peripatus* and other interesting animals, which he brought home for his Museum. On arriving home he tried to get into the army, but at 42 years of age found it difficult. He assisted with the formation of a Company of the Royal Engineers stationed at Wantage Hall, a residence for male students of Reading University College, and later he was commissioned in the 4th Territorial Battalion of the Essex Regiment. For the remainder of the war years he was stationed at Great Yarmouth and Caister in charge of a coastal gun emplacement. His scientific accuracy in shooting was based on a detailed plotting of the coastal area by means of a tacheometer which he purchased at his own expense. Bracketing a target was not the highest level that he thought possible in gunnery, and he is reported to have hit with his first shot a target towed by H.M. Navy for a test shoot.

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* *Nature*, 1939, **143**, 368 said that it was without a rival among those of the smaller Universities and had recently been called the 'gem' of the British museums of comparative anatomy. A fuller account of the Cole Museum was published in *Nature*, 1956, **177**, 555-556.

Returning to Reading after demobilization, he leaned more and more towards the history of biology as his way of research, and for this his strong literary bent, and the library which he had been building up ever since he was sixteen years of age, were admirable equipment. He realized that to write history and make it interesting required knowledge, accuracy and style. The first two he had to a supreme degree; style he cultivated by studying the masters of English prose, in particular Edward Gibbon (1737-1794), whose *History of the decline and fall of the Roman Empire* he read many times. After the manner of Gibbon, in his own writing he 'relieved the tedium of instruction with an apt analogy or an appropriate anecdote'. He could speak no foreign language, but could read French, Old French, German, Latin, and Old Dutch.

In May 1925 he delivered two lectures at King's College, London, on 'The history of protozoology', and these were published by the University of London Press in the following year.

In 1926 he was elected to the Fellowship of the Royal Society. This was the year of the general strike, in which he served as a special constable, and contact with the police inspired in him a profound and lasting respect for that force. Also in this year University College, Reading, received its charter and became the University of Reading. Cole had had much to do with drawing up the statutes, and he had enlisted the help and advice of men learned in university organization, such as his old friends Mackay and Elton from Liverpool.

In 1930 he published at the Clarendon Press his *Early theories of sexual generation* as a relaxation from the ties of a more exacting work on the history of biology on which he had been engaged for many years.

By this time his library was increasing at such a rate that more room was necessary to house it. So he bought a piece of land near Peppard Common in Oxfordshire, and on it he had a house built, literally round the book cases. At the time of his death he had nearly nine thousand volumes dealing with the history of zoology.

His son, Frank, as mentioned above, had gone earlier to Canada and became a farmer near Hamilton, Ontario. His parents paid five visits to him in that country, and in 1936 combined a journey to Canada with attendance at the Tercentenary Celebrations at Harvard, where Cole represented the youngest British University. They also visited Yale, and Cole was overjoyed to meet a co-follower and collector of the works of Vesalius in the person of the great brain surgeon, Harvey Cushing.

A study of the letters of Antony van Leeuwenhoek brought Cole in touch with Clifford Dobell, and in 1937 the former published two papers on Leeuwenhoek's zoological researches. He visited Holland and met many Dutch zoologists, and he was specially interested in the Museum of the History of Science at Leyden, 'which houses a magnificent collection of manuscripts and instruments associated with the discoveries of Huygens, Swammerdam, Leeuwenhoek and other scientific workers'. On his return he had a replica made of Leeuwenhoek's simple microscope.

Having failed to find a copy of the publication of the 'Private College' of Amsterdam, which is the rarest and least known of all the early literature on comparative anatomy, Cole decided to print an exact copy of it. This was done in the Typography Department of Reading University, under the direction of the late Robert Gibbings, Cole contributing an explanatory introduction. A numbered edition of a hundred copies is now almost sold out.

In 1939 he gave a very interesting talk to the Oxford Bibliographical Society, entitled 'Bibliographical reflections of a biologist'. It was published in the *Proceedings* of the Oxford Society, and aroused such interest that it was reprinted in *Osiris* in 1948.

Ten of Cole's students distinguished themselves in zoological research; six of them were awarded the degree of D.Sc. of London University and two of Reading University. Nellie B. Eales produced a monograph on the anatomy of the foetal elephant, and for it was given the Makdougall-Brisbane Prize and Gold Medal of the Royal Society of Edinburgh. Doris R. Crofts, Reader in Zoology at Queen Elizabeth College, University of London, made distinguished contributions to the embryology of gastropod molluscs. Ethelwynn Trewavas specialized in ichthyology and is now Deputy Keeper of Zoology at the British Museum (Natural History). Eric T. B. Francis, Reader in Zoology at the University of Sheffield, wrote a monograph on the salamander. Thomas H. Taylor, as an entomologist, did important work in the economic field in Fiji and Africa, and has recently been appointed Director of the Anti-locust Research Centre. Phyllis A. Clapham is a research parasitologist at the Wellcome Research Institute at Frant, Kent. Scott G. Cowper, also a parasitologist, qualified in medicine after graduating in science, and has held research posts in Mauritius and Nigeria in connexion with the campaign against schistosomiasis. Eric Burtt, an entomologist, worked on the trypanosomes causing sleeping sickness in East Africa. David Tompsett wrote a monograph on *Sepia* and is Prosector at the Royal College of Surgeons. Mrs Tattersall (O. S. Attride), in conjunction with her husband, produced the Ray Society monograph on the Mysidacea.

Cole retired from the Chair of Zoology at Reading in September 1939 at the age of 67. His colleagues and students past and present gave him a copy, painted by Willem Stad of Amsterdam, of Jan Steen's 'The Doctor', a picture he had known and admired during his visits to Holland. His portrait by Howard Coster was placed in the Zoology Museum, which the Council of the University decided should henceforth be known as the Cole Museum of Zoology. He became Professor Emeritus in 1940.

Relieved after so many years of the duties of a university professor, Cole settled down in his country house, at that time without any other near it, among his books. He spent several years preparing a card catalogue of the *contents* of all the volumes in his library, a most valuable working scheme. He was also writing his *History of comparative anatomy from Aristotle to the eighteenth century*, which was published in 1944 by Macmillan & Co. Ltd, and reprinted in 1949. In 1951 he delivered the Wilkins Lecture of the Royal Society on

'The history of micro-dissection', and in 1953 he contributed 'The history of Albrecht Dürer's rhinoceros in zoological literature' to the volumes written in honour of Charles Singer. For the Harvey Tercentenary he wrote on Harvey's animals. His last publication, *Obiter dicta bibliographica*, issued at the age of 85, tells the history of some of his successes and failures in book-buying, and all who knew him can visualize the charm, humour and erudition of the writer.

On 10 January 1959, after a quiet day in his library entertaining a friend, he had a stroke. He was found in the room in question by his corgi, which summoned help, and next day he was taken, still in reasonably good condition, to Park Hospital, Reading. On 27 January, however, he suffered a second, and immediately fatal, stroke, a week before his eighty-seventh birthday.

He wished that some part of his body could be made use of after his death, and this was duly done through a gift to the local eye bank.

Of Professor Cole it can be said that whatever he attempted he did superbly well. His scientific work showed meticulous care, and the illustrations exhibited his fine draughtsmanship. He would discard a drawing and begin all over again if he made one false line. No student of zoology who sat under him will ever forget the inspiration of his lectures. He was incidentally an authority on ecclesiastical architecture, and his book on Cholsey Parish Church contains magnificent photographs; in it he applied scientific methods with conspicuous success to the solution of an architectural problem. Though not a performer, he was learned in classical music, with a particular leaning to that of Johann Sebastian Bach. He loved pictures, especially those by the Dutch and Flemish masters in British, Dutch and Belgian art galleries. He studied literary style in the great English prose masters, and wrote with ease and elegance. Books he collected throughout his life, from school days to the time when he could acquire valuable early works on zoology and comparative anatomy from booksellers all over Europe.

His massive head, humorous brown eyes and artistic hands gave him an air of distinction that drew the attention in any gathering of scientific men. Though he might have seemed to some to be severe, he was in fact most kind and friendly, enjoying the company of young and old, entertaining them from his vast knowledge of men and books, his fund of good stories and his delight in his animal pets. He was at his best on field excursions with his students, who became his friends for life. They admired and loved him, and tried to follow the standards which he had set for them 'in industry, craftsmanship and unprejudiced seeking for truth'. On his seventy-fifth birthday a group of his students who held posts in zoology and had engaged in research sent him an address signed by all of them, recording their gratitude for his friendship, hospitality and help in their careers.

He was a man of simple tastes and led the austere life of the scholar. He disliked fighting, but would take up the cudgels on behalf of a colleague or a student to right a wrong. He had the lawyer's abstract sense of justice and though he was not quick-witted in repartee, he could see to the heart of a problem and stand up for his convictions. His advice was sought by young and old in the University.

He had great charm of manner and acted always with humility, unfailing courtesy and dignity. A working man said of him: 'He never paraded his knowledge or condescended to those who knew less than he did', while his immediate successor as Professor, Charles H. O'Donoghue, said: 'He was a quiet, retiring man, not suffering fools gladly, but ready to share his knowledge with student or friend. So wide was this knowledge that an evening spent in his pleasant company was always something to remember'.

It has been a very great honour to be asked to prepare this biographical memoir of an old friend, but I am very conscious of what it owes to the carefully thought-out and written notes which he himself left with the Society, and also of the unstinted further help which has been given to me by Miss Nellie B. Eales, D.Sc., who was his Senior Lecturer in Zoology, and who lived with his wife and him for nearly thirty-nine years.

K. J. Franklin

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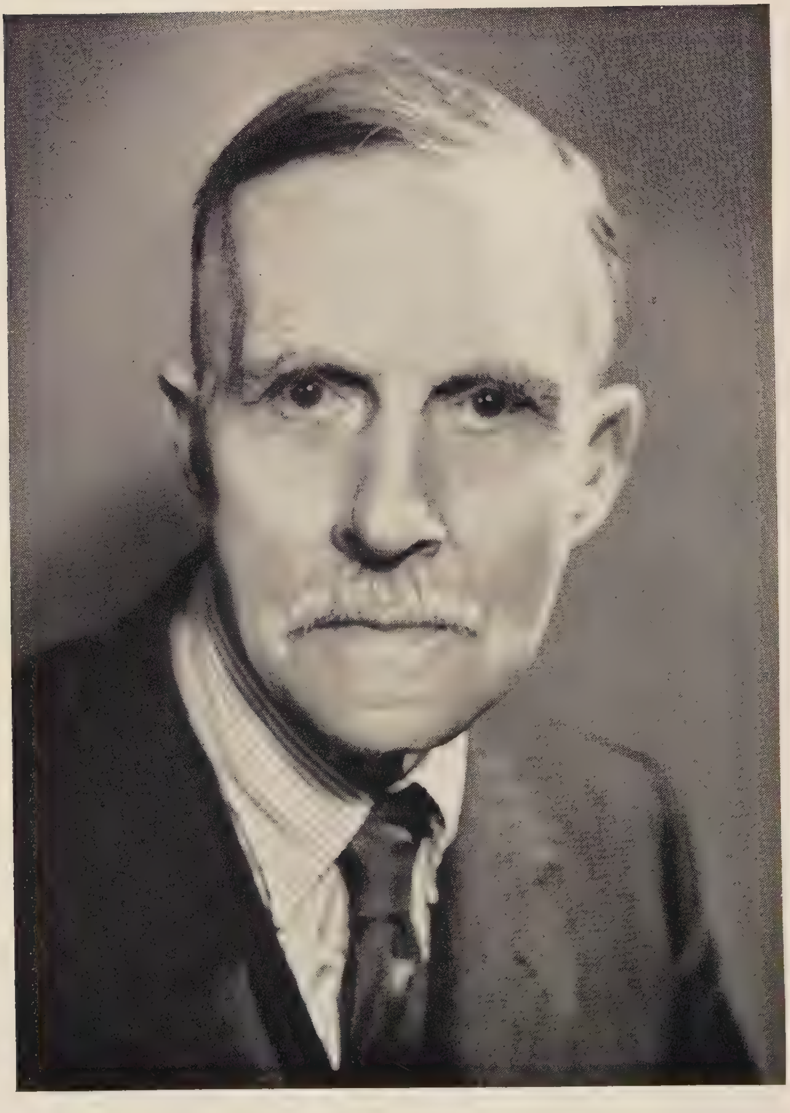

Cecil H. Desch.

CECIL HENRY DESCH

1874-1958

At the beginning of this century the study of metals and their alloys was given a new interest and direction by the work of Floris Osmond in France and Roberts-Austen in this country. Prior to that time the chief concern of metallurgists lay in the extraction of metals from their ores, which had been won from the earth by the miner, and so mining and metallurgy worked very closely together. The physical properties of the metals themselves and the manner in which improvements in these properties might be achieved was knowledge in the possession of practical people and the basis of that knowledge was almost wholly empirical.

The work of Osmond and Roberts-Austen inaugurated and inspired a real scientific approach to the study of metallic properties and truly founded scientific metallurgy as we know it today.

Few metallurgists who were trained in their subject during the past half-century have not had cause to be grateful for the admirable survey of the science of metals as it was developing during that time contained in the *Textbook of metallography* written by Cecil Henry Desch and first published in 1910. That this book went through four printings and was then rewritten and issued as a fourth edition in 1937 shows how the demand has continued over the years and how greatly its value has been appreciated by succeeding groups of students.

Cecil Henry Desch was born on 7 September 1874 in London—the son of Henry Thomas Desch and his wife, Harriet Ingerson. When only a few months old Henry Thomas Desch had the misfortune to lose his own father, a seafaring captain who had died of cholera, and he was brought up at Boughton in Northants where he attended a local school near Northampton. The teaching was surprisingly good and the master seemed to have taken a special interest in him. At any rate when he found that his pupil was interested in measurement, he proceeded to give him special coaching in land surveying which enabled him on leaving school to become an articled apprentice with a firm of surveyors. On completing his articles he joined W. Cubitt & Co., Grays Inn Road, with whom he remained for 52 years, attaining the position of Chief Surveyor, and for the last few years was a Director of the company. During his connexion with this firm he was responsible for many important constructional contracts including the Cunard Building in Liverpool, important offices in Cornhill and Lombard Street, and a large number of private country mansions. During the First World War he also had charge of a factory associated with Cubitts for the manufacture of shell fuses, and the strain of this extra work undoubtedly affected his health. Earlier in his career H. T. Desch had been sent over to France to study cements and mortars in connexion with his work as surveyor and builder, and during his stay in that country his interest in the French country dialects was aroused. That interest became a great hobby and he never missed an opportunity of travelling to France to add to his knowledge of the subject. On many of these occasions he took his young son, Cecil, with him, and these visits laid the foundation for that excellent knowledge of the French language and that facility in speaking it which brought a great deal of pleasure in later life. His many contacts in that country made the name of C. H. Desch a well-known and much-respected one in French scientific circles. (He had the honour of being elected a corresponding member of the French Academy of Sciences in 1947.)

While intensely interested in the subject of dialects, his father never had the facility nor perhaps sometimes the courage to pronounce some of the unusual sounds and one of the reasons why he liked Cecil to accompany him on such travels was the help he got on difficult occasions through having someone with him who would face up to the problem without hesitation.

Cecil's mother, who was born Harriet Ingerson, came from Pilton, N. Devon, and was the youngest of eleven children, all of whom lived to a good age. Later, as governess-companion to Mrs Howard Vyse, whose husband was the Vicar of Boughton, she accompanied Mrs Vyse on many continental journeys to the South of France where a part of each year was always spent. It was at Boughton that she met H. T. Desch, and on their marriage they came to live in Tottenham, Middlesex, at that time almost rural on the fringe of the countryside surrounding London. Mrs H. T. Desch died in 1933 at the age of 90 and to the last her memory of all the events in her long life was clear and unclouded. The family consisted of two sons and one daughter, the brother of Cecil, Ernest Conway, carrying on the calling of their father as a surveyor.

It was from his mother that Desch received his early education which she carried on until he attained the age of nine when he was first sent to a school. With the aid of William Ellis's phonetic system she taught him to read and from her he acquired a knowledge of botany which remained an interest throughout his life. A walk with him through the countryside was made more interesting by his identification and comments on the wild flowers that were passed on the way and the numerous varieties of alpine flowers always gave him special pleasure. His home training was broadened by visiting teachers in French and music, and his ideas on religion were guided by the knowledge to be gained from such books as Edward Clodd's *Childhood of religion* and Moncure Conway's *Sacred anthology*—an anthology drawn from all the religions of the world.

His father's attitude to religious matters was in this way clearly shown.

Although in his earlier years Henry was a member of the orthodox church, he was a seeker after truth and he later came to hold the view that 'nothing should be believed that could not be clearly proved'. He was a frequent attender at lectures on ethical and sociological subjects at Conway Hall to which his young son, Cecil, frequently accompanied him. With a deep admiration for his father, it was natural that Cecil should grow up with a mind that viewed religious convictions with an open detachment.

When nine years old he went to St John's School, Tottenham, where the teaching does not seem to have been particularly good. Map drawing was an important subject in the curriculum and much time was devoted to it. That and the exercise of committing to memory long lists of Latin and Greek roots seemed to be the only subjects that left any impression of the time spent at that school. Desch did admit, however, that his knowledge of Latin and Greek roots came in useful later on. In sketching buildings and landscape scenes he possessed a quite unusual skill and he made many beautifully executed drawings of botanical specimens, archaeological objects and historic buildings for his own records. So perhaps the exercises on map drawing were not altogether without their value.

Three years later he was transferred to Birkbeck School, Kingsland, a school founded by William Ellis to give secular teaching only. There was a strong bias towards science subjects which included chemistry, physics and physiology. There was also a subject referred to as 'social science' made up of a blend of economics based on the ideas of John Stewart Mill, combined with sound teaching on the principles of citizenship. The headmaster, James Runtz, had been a pupil of Huxley and he was an admirable teacher. He held the post of headmaster to that school for 52 years.

This school laid little stress on examinations and did not enter the boys for scholarships. Nor did it offer any opportunities for games, although there was a good gymnasium and some military drill was given. But there was a chemistry laboratory and the teacher of chemistry, William Runtz, a nephew of the headmaster, had been trained at the Royal College of Science at South Kensington. He was an enthusiast and he aroused the interest of the pupils in his subject with the result that many of them decided on chemistry as their future career. In addition to the chemical laboratory, there was a good collection of minerals and natural products used in industry whilst, to assist in the teaching of physiology, there were such things as a skeleton which had been obtained from Germany with detachable heart, lungs, brain and eyes, and large coloured physiological diagrams which had been prepared at the instigation of the headmaster. Although no religious instruction was given, secular moral instruction was given and the moral standard of the school was high. Desch never regretted his parents' choice of this school and there is no question that his own interest in science was awakened by the instruction which he received there and by the enthusiasm for the subject in those who taught him.

The neighbourhood of Kingsland gradually changed. The houses were broken up into tenements and more and more the pupils came from outside the district. On the death of James Runtz the school came to an end and the buildings and equipment were handed over to the London Education Authority to be used by them as a training centre for teachers.

On the break-up of the school some of the pupils attended evening lectures on chemistry at Toynbee Hall and through this contact they gained some knowledge and insight into the social work being carried on at that centre. During his last year at school Desch was head boy, which was an early indication of that recognition by his fellows of his individuality and his ability.

Having decided to study chemistry, he entered at the early age of 15 the City and Guilds Technical College at Finsbury. This undoubtedly was the main formative influence on his scientific career for he enjoyed every day of the time that he spent there. At that period Finsbury Technical College was at the height of its fame. Under the guidance and direction of Sylvanus Thompson, as Principal and as lecturer in physics and electrical engineering, with John Perry who taught mathematics and mechanical engineering, and Raphael Meldola as the Professor of Chemistry, the College was fortunate in possessing a group of brilliant teachers. But they were more than teachers, they were strong personalities in their own right. Thompson, a brilliant lecturer with a most attractive and pleasant manner and a wide range of knowledge in other subjects than his own; Perry who possessed a highly original mind and a flair for presenting subjects in an unconventional manner, and Meldola thoroughly conventional but thoroughly sound in his grasp and knowledge of chemical science. These men gave inspiration to more than one generation of students who gave them their respect and admiration in return.

Desch obtained first place in the College examinations after spending two years as a student in organic chemistry and remaining for a further year in Meldola's laboratory as a research student. His work during that year resulted in the publication of a paper under their joint names on 'Some homonuclear tri-derivatives of naphthalene'—his first scientific paper published at the early age of 18.

The demonstrators at that time in the Chemistry Department were also particularly good teachers. They were John Castell-Evans, a slightly eccentric Welshman, and Frederick William Streatfeild, to whom many prominent English chemists owe their introduction to organic chemistry. Amongst those were G. T. Morgan and M. O. Forster who had preceded Desch in the course at Finsbury the previous year and who both became professors at the Imperial College of Science and Technology when that College was inaugurated.

Desch had now confirmed his decision to follow chemistry as a career and he discussed with some of his fellow students who had arrived at a similar decision the best way of obtaining further experience and training. M. O. Forster strongly advocated a stay at one or other of the German Universities which were pre-eminent throughout the world as centres of chemistry research and teaching, but to Desch the problem of financing a stay of any duration in Germany proved to be too difficult and he had regretfully to abandon the idea for the time being. The desire to do so however remained strongly in his mind and it was again Forster who recommended, with some knowledge of the financial problem, that Wurzburg should be considered as it was one of the less expensive centres with an active chemistry department.

In the meantime some remunerative work had to be obtained and, following a reply to an advertisement, a position as an assistant chemist was obtained with F. Kendall & Son in Stratford-on-Avon. This firm manufactured chemical materials for the brewing industry and during his association with this works Desch was engaged on the routine testing and control of the processes for the manufacture of such materials used in brewing as caramel for colouring, bisulphate for sterilization and non-fermenting sugars like levulosan, which were at that time the mainstay of the business. Some research work was also carried out and during his period as an assistant chemist this research led to the granting of patents for the production of caramels which were soluble in spirits and to the publication in the *Chemical News* by Desch of one or two minor papers and notes. The bacteriological examination of yeasts and ferments was also carried on at the Stratford laboratory and photo-micrographs of samples were taken and recorded as a matter of routine control. In any work in which he was engaged, Desch had as a natural habit the desire to study and to make himself familiar with the published information on every aspect of the subject. During this period he acquired as a consequence of the work in Kendall's laboratory a good knowledge of bacteriology and, rather surprisingly, a practical understanding of those defects in the product which arise during the brewing of beer and a knowledge of the various ferments which cause them.

What proved of greater significance later on was the interest which developed into photo-micrographs and microscopes and the practical techniques for successful high-magnification photographs, for it was from this beginning that he turned to metallography with the wish to apply these same techniques to the study of metal surfaces. Desch always said that his interest in metallography started during his Stratford days.

Among the commissions received by the laboratory, in which Desch took a leading part, was one to prepare small quantities of all the rare earths and particularly scandium, which had been requested by a man who ultimately turned out to be a company promoter. Many months were spent on fractionating salts from Norwegian minerals and monazite sands from Ceylon and there are records to show that the rare earth content was estimated from the absorption spectra of solutions with the aid of a Browning spectroscope—a very early example of the use of a spectroscope for the purpose of quantitative analysis. In the end the work was abortive for it ultimately became known that the object of all this research was an endeavour to find a way of evading the Welsbach patents for gas mantles. In this the company promoter was not successful and an action for infringement was raised at which Desch had to give evidence as an expert witness. He recalled the occasion always with an amused chuckle for he said that his only contribution to the trial was to convince the learned Judge that elements like scandium did really exist and were not merely figments of the imagination.

Many requests came to Kendalls for standard solutions and for reagents for testing as at that time the brewers were very dependent on German chemical manufacturers for their supplies. This led the company to start a department for the manufacture of fine chemicals and in the development of this section Desch took an active part. The new development was widely advertised. Kendalls announced they could undertake the making and supply of chemicals not otherwise obtainable with the result that this somewhat rash assertion led to a large number of orders for chemicals which it was quite unprofitable for a small department to produce. Orders for many unusual organic compounds were taken and their preparation gave rise to work which, while intensely interesting to a chemist, was financially unremunerative to the company. The experience which Desch gained in such work proved most useful when he was requested during the Second World War to assist the Government, at the instance of the Royal Society, by organizing a team of workers for the preparation of chemicals no longer obtainable.

About this time however Desch began to feel that his future advancement might be hampered by the lack of a university degree. The spare time of his evenings was therefore given over to continuing his studies and he worked for an external degree from London University, passing successively the matriculation and the Intermediate B.Sc. examinations. In addition to mathematics, physics and chemistry, he had to satisfy the examiners in botany and zoology as well. Without a teacher he taught himself the dissection of plants and animals, and he worked assiduously through all the standard types. As if this was not in itself enough, for his principal subject in the final examination he chose geology. Extensive study was made of the exposures in the neighbourhood of the local Lias deposits and the collection and examination of fossils were added to his activities. This new interest was probably an advantage to his health for it took him on long walking tours and gave him an opportunity for exercise which he would not otherwise have taken.

At that period of his life he drove himself hard and it was fortunate that his constitution survived without any apparent damage. Still set on going to Wurzburg, he saved money with unrelenting determination and even on the matter of food the minimum sufficed. He took time for some relaxation, however, through his interest in Shakespeare's plays and in the Shakespeare Memorial Theatre of which he was a most enthusiastic supporter. The Memorial Library was another favourite haunt and he had an extensive and understanding knowledge of the dramas about which he contributed papers on several occasions to the Shakespeare Club. Stratford-on-Avon was an active and interesting period in his life and Desch retained many happy memories of his stay in the town.

In the final examination for the London B.Sc., he not only passed successfully, but he won a scholarship of £40 tenable for two years.

Meantime the Department of Fine Chemicals at Kendalls was not overcoming its initial want of financial success and it was decided to close it down. The staff had to be reduced and, to save an older man from having his employment terminated, Desch volunteered to leave of his own accord. With what he had saved and with the help of his scholarship, he thought he had sufficient money to take him to Wurzburg and there he proceeded in 1900, having spent eight years in the service of Kendalls.

There are few records of the time spent at Wurzburg University. The Professor of Chemistry was Arthur Hantzch under whom he worked for the Ph.D. degree. It was necessary to take two other subjects in addition, so physics was studied under Willy Wien and botany under Emil Kraus. His research work was concerned with organic compounds of ferric iron and it earned him his degree in 1902 *summa cum laude.*

Returning to England and somewhat uncertain what his next move should be, he was advised to consult Sir William Ramsay who was then the Professor of Chemistry at University College, London. Their interview was brief and to the point. After talking to Desch for a short time on his work in Germany and his ideas on the future research he wished to carry out, Ramsay said 'All right, Desch, you can start in the lab. tomorrow.'

The subject which interested him most and which he had touched on in Wurzburg was that of tautomerism and he resumed its further study at University College. Another occupant of the laboratory was E. C. C. Baly with whom he discussed the project. Baly suggested that it might be attacked with the help of ultra-violet absorption spectra and the research was commenced in that way, first with a large grating instrument but quickly changing to a quartz prism instrument supplied by Hilger. It was found that the interesting part of the spectrum was confined to a narrow region and after much detailed work a relation between the absorption by a layer of known concentration, or rather the logarithm of the thickness of the layer, was worked out only to find when it had been completed that the basic theory had already been published by Rayleigh. A number of joint papers with Baly were however published and later further publications on the same subject took place in conjunction with T. M. Lowry who also worked in Ramsay's laboratory at University College.

This continuance of the work started at Wurzburg led to the award of the D.Sc. from London University for a thesis on 'Keto-enol tautomerism' in 1902.

An opportunity to apply his knowledge of metallography occurred when Sir William Flinders Petrie, who was on the staff of University College, asked him to examine the composition and structure of metal implements which had been found during the excavations at Ur. This was done with his usual thoroughness and an interest in archaeology inevitably followed. That his knowledge of the subject was far from superficial was recognized when later he was asked to become a member of the British Association Committee on that subject. A subject of much interest to that Committee was the origin of the Sumerians and it had been suggested that an analysis of the copper and bronze objects found on the sites which had been excavated might throw light on the problem. If it were found that they contained impurities which were characteristic of those metals in other parts of the habitable area known at that time, some light might be thrown on the sources from which the metals were derived and consequently on the migrations of the Sumerians themselves.

Desch undertook the analysis of many objects for this purpose, first by the ordinary chemical methods and latterly as an added refinement by spectroscopic methods. He became secretary of the Sumerian Committee and several reports were issued as the work progressed. The curators of various museums became interested and submitted examples of other copper and bronze objects and the scope of this Committee widened and the work increased. That this activity was appreciated can be indicated by the many centres which took advantage of this new approach to obtain more information about the archaeological objects in their charge. In addition to the British Museum, work was done for the Louvre, the Oriental Institute of Chicago, the Iranian Institute of New York and many other bodies. To help in the analyses, an assistant was appointed and the cost was most generously financed by Sir Robert Mond. Unfortunately on his death this help ceased and the work of the Committee came to an end. After being in abeyance for some time, it is of interest to record that the Copper Development Association assumed the responsibility of restarting the work and now carry it on.

To this phase of his work Desch ascribed the origin of his keen interest in the history of science, but it was also fostered by the teachings of Positivist Philosophy and by his friendship through this connexion with other members of the group like F. S. Marvin. This author, who had published *The living past* and other books interpreting historical development, ran summer schools in Rome and other places at which Desch was a frequent lecturer.

The award of the D.Sc. marked the end of his training period and the need to seek some remunerative employment now arose. At King's College, London, the Professor of Metallurgy, A. K. Huntington, had just bought a metallographic microscope and wanted an assistant who had some acquaintance with the instrument. Desch received his appointment and for the next few years his growing attachment to this subject ran parallel to the interest in organic chemistry in which all his training had been centred. He acted during this period as assistant sub-editor to John Greenaway for the Chemical Society. Large numbers of journals had to be issued to abstractors, the abstracts they sent in were corrected, set up in proof and the proofs corrected. He always regarded this experience as giving him an excellent training in the use of scientific literature. The habit of making an abstract for his own use of any article or paper that he read and wished to retain for future reference remained with him all his life.

For the Chemical Society he wrote for many years the annual progress reports on organic chemistry, at first alone but latterly with collaborators. Abstracting, in fact, was a means of augmenting the rather meagre salaries paid in those days to scientific assistants and Desch did much of this work. When the Institute of Metals was founded, he did much abstracting for it as well as for the Iron and Steel Institute. Articles for the technical press were another source of income and his many-sidedness was shown by several articles contributed to the *Concrete and Constructional Engineering Journal.* The work of his father, which involved reinforced concrete construction, no doubt gave this subject an added family interest.

Desch did not find it very easy to work with Professor Huntington who was quick-tempered and exacting. Although the detailed investigation work had been carried out by assistants, he was averse to publishing joint papers of the results in which their names would have appeared with his. He was an ardent balloonist and the cellars of the laboratory at King's College were packed with varieties of balloon fabrics, all undergoing testing and examination. Indeed, for some time after taking up the post, Desch was engaged in investigating all manner of resins to find better ways of making balloon fabrics less permeable.

Working in the chemical laboratory at this time was another assistant, Miss Elison Macadam, who was engaged in the accurate analyses of metal samples which were then examined metallographically by Cecil Desch. The daughter of William Ivison Macadam, Professor of Chemistry at the College of Surgeons, Edinburgh, she had found she was debarred from taking up a chemical career at Edinburgh University because women students were not admitted. So she entered King's College, London, and trained for the Institute of Chemistry examinations under Thompson and Herbert Jackson who had recommended her to Professor Huntington when her examinations were successfully passed. Her meeting with Cecil Desch was a happy circumstance for both, for during 1908 they became engaged.

The work at King's College offered little prospect for further advancement. The salary was only £150 per annum and, even with the help of outside work, this was not sufficient to face the responsibility of a new home. So Desch resigned and started looking around for a better paid position. This was probably the most trying period of his life. New jobs were not so easy to get, nor did he find disappointments easy to bear. That most keenly felt was the appointment of another candidate to the secretaryship of the newly-founded Institute of Metals, but shortly afterwards he applied for and received the Graham Young lectureship in metallurgical chemistry at Glasgow University, thus starting on the career in which he found both satisfaction and success—that of university teacher.

In January 1909 Cecil Desch and Elison Macadam were married and those who were later privileged to visit them will always recall with pleasure the happy atmosphere of a home created by a devoted partnership. Professor Huntington remarked regretfully that Cecil Desch had robbed him of his best assistant.

The post in Glasgow was nominally under Professor John Ferguson, the Professor of Chemistry and the learned historian of everything pertaining to that subject. A fortunate degree of freedom however was allowed the lecturer. The classes were small, there was a good metallographic outfit and several on the staff of the Engineering Department came for instruction in its use. There was time also for research, and work on corrosion was started. Time was available too for collecting the mass of information that went into the writing of the famous *Textbook of metallography* which came out in 1910 and immediately won recognition.

After the first year, however, the classes were reorganized, the engineering students being separated from the main chemistry class which included those taking pure science and medicine, who received their lectures from Professor Ferguson. Desch took the engineering students who came from many parts of the world. With many Indians (including Sikhs complete with turbans) were Chinese, Philippinos, Egyptians, Norwegians and Italians, so above everything a lecturer had to be clear and unambiguous. Desch took great pains with his lectures and devoted much thought to the manner of their delivery, thus laying the basis for a lecturing style that became altogether admirable during the progress of his career. He had the ability to explain in clear and lucid terms the scientific principles underlying any new advance in metallurgical thought and he could always bring to bear in a discussion an extensive knowledge of the available literature.

At that time a close neighbour at the University was Sir George Beilby whose interest in metals and particularly in the surface changes which occurred when metallic surfaces were polished had led to the publication in the *Transactions of the Royal Society* of papers which had greatly advanced our knowledge not only of the polishing process but also of some of the more profound aspects of the metallic state. This common interest fostered between the two men warm feelings of respect and friendship.

In the May Lecture to the Institute of Metals in 1911, Beilby referred to a theory of Quincke that all solidification was preceded by separation into two states, one of which formed foam-cells within which the other part solidified. A committee was set up by the Institute to formulate this idea into a research project and to state the questions to which answers were desired, and Beilby offered to finance the enquiry. Desch was appointed official investigator to the committee and at the same time the scope of the enquiry was widened to cover the general question of solidification from the liquid state.

The First Report by Desch to the Beilby Research Committee was a masterly summary of the then existing knowledge. A very large number of sources had been consulted and all essential information set out in a clear and lucid manner. In addition to Quincke's hypothesis, attention was directed to other ways in which a cellular structure in solids might arise, and the effect of surface tension and other physical properties came under review. In the Second Report, experimental results on some aspects of grain formation and shape were given. From an experimental study of metallic masses, which in alloys like brass could be separated into their constituent grains, Desch showed that there was indeed a close analogy between the distribution of faces in foam grains whose form had been determined by surface tension and metallic grains where surface tension effects were secondary, but both were in reality derived from a more general principle which had previously been studied by Lord Kelvin, namely, the homogeneous division of space into cells without forming voids. Desch then went on to study convection cells, but came to the conclusion that they were of little importance in the solidification of metal masses.

These reports to the Beilby Prize Committee attracted a great deal of attention and were widely praised for the balanced judgment with which a large amount of information bearing on the solidification of metals had been marshalled and critically surveyed. They did much to establish the reputation of Desch as a scientific metallurgist of the first order.

He enjoyed his association with Glasgow University and when he was invited to occupy the Chair of Metallurgy at the Royal Technical College, which became vacant in 1918, he very willingly accepted. At the same time he strongly recommended that the two courses should be merged and that the Department of Metallurgical Chemistry with its equipment should be transferred to form a unified metallurgical centre. This was agreed, but there was a period of transition during which, while Professor at the Royal Technical College, he continued to give lectures to the chemistry classes at the University. This was mainly brought about by the fact that Professor Ferguson had retired during the war period of 1914-18 (he died shortly afterwards) and no immediate appointment was made to the vacancy. These classes were something of a trial. They numbered several hundred students and during Professor Ferguson's time a tradition of rowdiness had grown up which for some time continued unabated during Desch's lectures.

The work of the metallurgical department at the Royal Technical College was heavy. It included evening work to a certain extent as well as day work and there was little time left for original research. The numbers in the classes were greatly increased by the intake of students whose training had been interrupted by the war and who were now desirous of resuming their studies. The atmosphere was however a helpful and pleasant one, and Desch introduced a new and fresh outlook into a department which had been carried on along somewhat old-fashioned lines.

In 1920 the Chair of Metallurgy at Sheffield University became vacant through the retiral of Professor J. O. Arnold and Desch received an invitation to become a candidate for the vacancy, to which in due course he was appointed.

Once again he had to tackle a job of major reorganization for Arnold had no great liking for theoretical studies and had carried on the work and teaching on the basis of traditional knowledge obtained from the observational powers of skilled workers in the metallurgical art and he had quite neglected the great advances in scientific thought which were then taking place. In a city whose prosperity was centred on the metallurgical industries, this was unfortunate, and shortly after Desch arrived a conference of old students and those interested in the University was called to discuss ways and means for improving and modernizing the teaching of metallurgy in the department. This proved to be most helpful and plans were drawn up and were ultimately put into effect for a complete reorganization. Desch's colleagues were also helpful and he was greatly indebted to William Ripper, Professor of Mechanical Engineering, and to Dr Fred Ibbotson in his own department for all the assistance they gave him. Through the generosity of Sir Robert Hadfield and other Sheffield industrialists, an extension to the laboratories was made possible and much-needed equipment also provided.

With his growing interest in metallography, Desch found his transference to Sheffield a change filled with admirable opportunities. The founder of metallography, Henry Clifton Sorby, was a Sheffield man and, although he had never occupied any academic position there, he left his collections of specimens to the University. He had also left a tradition, for his micrographs were produced before the advent of any skill in microphotography and they were all drawn by hand. With one eye glued to the microscope and the other focused somewhat indefinitely on a sheet of white paper, the research worker in his reproductions of metal structures had ample opportunity during his drawing of them to impress the details firmly in his own mind as well. Desch always advocated this practice for students as the best way of teaching this subject at the beginning.

On his first arrival in Sheffield he found the tradition of the Sorby method still very much respected and Dr Ibbotson had great skill in the reproduction of metallic structures in this manner.

When one regards the enormous extent of the practice of metallography today, it is well to remember that its introduction by Sorby to the Geological Society in 1857 did not escape critical comment. In discussing the subject, one of the Vice-Presidents, Leonard Horner, remarked that 'he had been a member of the Geological Society ever since its foundation and during the whole of that time he did not remember any paper having been read which drew so largely on their credulity'.

Desch enjoyed his association with Sheffield and its University. There was the most friendly co-operation with the steel industry of the city and, although he always refused to undertake consulting work for fees, he was very willing to give help and advice to anyone who asked for it. He loved rambling over the Derbyshire moors and on fine Sundays he would arrange for a party of colleagues and students to meet at one of the railway stations in the adjacent countryside as the starting point for a long and friendly walk. In promoting the social activities of the University, Mrs Desch took an active part. With enthusiasm and energy she founded a social club which soon became the rallying point for all their colleagues. It was very appropriate that the laboratory in the Department of Metallurgy should now be named the 'Desch' Laboratory and that a Desch Senior Research Fellowship should also have been founded to commemorate his association with the University and with the city in which he spent what was probably the most enjoyable twelve years of his life.

Attached to the Metallurgy Department was a small model works containing an open hearth furnace which was rebuilt and was used by Desch for teaching the practical side of steelmaking. It was useful, not only in carrying out experiments on steel compositions but in selecting those students who showed an instinctive aptitude for practical work and evidence of possessing managerial abilities. When the time came to take up careers on the completion of their courses, Desch was always of the opinion that this knowledge of student aptitude was of great use and enabled recommendations to be made for placing of men in their most appropriate spheres of work. In that sense it is a pity that the costliness of maintaining a furnace of this kind in a university establishment has led to the discontinuance of its use.

On account of the evening classes, grants were received by the University from the local Education Authority and these gave rise to a certain amount of friction with the Director of Education who seemed to find it difficult to understand why the cost of such courses should be higher at the University than in the local schools. This attitude caused Desch a good deal of uneasiness, for his nature was not of the kind that could turn to forceful rejoinder. So he suffered patiently and in silence until the opportunity for escape arose. This came along timeously when the National Physical Laboratory was looking for someone to succeed Dr Rosenhain as Superintendent of their Metallurgy Department, and after enquiries as to whether he would accept the post, he was appointed in 1932.

About this time also Desch received an invitation to deliver the George Fisher Baker Lectures at Cornell University and to occupy the corresponding lectureship for a semester of four months. He chose as his subject 'The chemistry of solids' and these lectures were afterwards published in book form and received widespread notice. The facilities which he enjoyed during his American visit of access to the well-equipped laboratories of Cornell and the opportunity it gave him of contact with the professional staff were greatly enjoyed. Whilst there other invitations were received and lectures were given at Yale, Harvard, Columbia and at most of the principal Universities in that country, and in this way be became well acquainted with the outlook and thinking of American academic life. Altogether it formed a most interesting and memorable interlude in his life.

On returning to this country he assumed his appointment as Superintendent at the National Physical Laboratory and he remained there until he reached the retiral age in 1939. He found a house at Wimbledon convenient to London where so many meetings had to be attended and convenient also to Teddington for his work. There was much administration required from the Superintendent and Desch found that this hampered the continuity of his application to the research problems. Nevertheless, the standard of the Department was well maintained and some members of the staff at that time have since achieved high academic distinction. One of the research studies in which he took a special interest was the determination of the oxygen content in steels and the fact that this difficult objective was attained by the vacuum fusion process during his tenure of the office always gave him satisfaction.

The outbreak of war in 1939 coincided with the time for retiral at the age of 65, but he remained for a few months longer to supervise some special investigations concerned with war work until his successor, Dr Sykes, could take over.

On his retirement Desch was appointed as a scientific adviser to the Iron and Steel Research Council, the forerunner of the British Iron & Steel Research Association, and a room was fitted up as a small laboratory in the building occupied by the Iron and Steel Institute. This was his headquarters for a few years and from there he gave much useful guidance and help to the research activities of the Council during a period when few people had the opportunity to devote much time to its work.

In 1943 Desch was asked to join Messrs Richard Thomas & Co. Ltd and was given a seat on the Board. His principal task was the establishment of a research laboratory in the works at Ebbw Vale and a building of very solid construction, formerly used as a blowing house for the blast furnaces, was reconstructed and allocated as premises for this new department. Before this laboratory was completed, however, the company amalgamated with Baldwins Ltd to become Richard Thomas & Baldwins Ltd by which name it is now known. Desch resigned his appointment on being asked by Mr G. H. Latham, the Chairman of the Whitehead Iron & Steel Co. Ltd to become scientific adviser to that company and he occupied this position for 13 years until ill-health made it no longer possible for him to continue. The kindness and co-operation which he received from Mr Latham and from everyone in the Whitehead organization was something about which Desch always spoke with great appreciation.

During the last years of his life he was handicapped by a long spell of indifferent health and he passed away peacefully on 19 June 1958, at 84 years of age.

With his wide range of interests Desch took an active part in many scientific and technical institutions and he was honoured by many awards and appointments. He was a founder member of the Faraday Society which elected him President in 1926-28, and also of the Institute of Metals where he occupied the Presidential Chair from 1938-40. The year after his demission of office, that Institute awarded him its Platinum Medal for his distinguished services to metallurgical science. An equally high distinction came to him from the Iron and Steel Institute by the award of the Bessemer Medal in 1938 and during 1946-48 he was President of the Institute. Elected to the Royal Society in 1923, he served on the Council from 1932 to 1934.

He was the recipient of honorary degrees from the Universities of Glasgow and Leoben and he was elected a corresponding member of the French Academy of Sciences in 1947 and made an honorary member of the American

Institute of Mining and Metallurgy in the same year. The Deutsche Gesellschaft fur Metallkunde elected him an honorary member in 1938 and he attended their annual conference in Munich that year, but the outbreak of war severed his connexion with that country and he never cared to renew it when the war ended. A member of the Court of the Blacksmiths Company, he took much interest in their training schemes and with his usual thoroughness he could speak with authority on the development and history of ornamental wrought-iron work, a subject in which he had long been interested. In the towns in which he resided he took an active part in the local scientific and technical societies and served on their Councils for a number of years.

The history of science also attracted Desch strongly and he was a member of the International Society for the History of Science and he served for a term as President of the Newcomen Society. Many of his lectures and addresses bear witness to this interest. He derived much enjoyment from patiently tracing back over the years the development of scientific ideas in which he was concerned from time to time.

As has earlier been said, Cecil Henry Desch acquired from the surroundings of his early years a belief in the rightness of the Positivist Philosophy of life and he never wavered from that attitude. Through the friendship of his father with Moncure Conway, who was then the pastor at South Place Chapel, Finsbury, he had many opportunities of listening to discussions on humanitarian subjects and the integrity and forthrightness of their views made deep impressions on his mind. His concern for sociology and his desire to understand its practical aims sprung from those very early years. This desire was greatly strengthened when he met Patrick Geddes in Paris in 1900 while attending a ceremony commemorating the achievements of Auguste Comte and he formed a lasting friendship with one whom he regarded as the greatest and most impressive lecturer of his time. When Geddes founded the Scottish College at Montpellier, he often invited Desch to visit him there and to lecture. An amusing incident once occurred. Desch received a letter in which Geddes wrote ‘If you don’t come to see me soon, I’ll be dead.’ Thinking that his friend was seriously ill, he hurriedly packed his bag and hastened off to Montpellier forgetting his overcoat and many other things he should have taken with him, only to find when he arrived that Geddes was in the best of health and had only used this manner of expression to convey to Desch that he had not seen him for some time.

When the Sociological Society was formed, Desch joined in 1904 and continued his connexion when it later became the Institute of Sociology, for many years being Chairman of Council. At that time the chief exponent of Positivism was Frederick Harrison with whom Desch was on terms of great friendship. Indeed, when he married in 1909, after the civil ceremony a Positivist ceremony conducted by Harrison also took place at Cliffords Inn.

At that period Positivist views exerted considerable public influence in Britain and Desch always viewed with regret the decline that has taken place since, a decline which he ascribed to a too rigid adherence by later members to the views of Comte without allowing for the great changes which have occurred in the intellectual and social developments of our lives since his death.

But throughout his own long life, there was no wavering, no deviation from the high ethical standards which he had accepted for his own guidance as a young man. He never questioned the beliefs of others, having 'sympathy with all who, whatever their beliefs might be, strove like himself to live faithfully by the best rule as they saw it'.

In the lists of Desch's interests, sociology took a prominent place and when to that subject there are added metallurgy, organic chemistry, physical chemistry, archaeology and science history, we can appreciate the extraordinary range of an exceptional mind. If a little austere in his outlook at times, he was nonetheless an interesting talker and was always a most pleasant and enjoyable companion.

He is survived by Mrs Desch, to whom I am indebted for much information, and by two daughters.

A. McCance

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T. L. Eckersley

THOMAS LYDWELL ECKERSLEY

1886-1959

Thomas Lydwell Eckersley was born on 27 December 1886 in London. His mother was a daughter of Thomas Henry Huxley who was at one time President of the Royal Society. From the age of $2\frac{1}{2}$ to 6 Eckersley lived in Mexico where his father, who was a civil engineer, was engaged in building a railway. In his early life Eckersley was interested in engineering and in scientific devices and he had a desire to emulate his father and to build bridges.

At the age of 11 he went to Bedales School where he came under the influence of an able teacher of mathematics who laid the foundations of his life-long interest in the subject. He left school at the early age of 15 and went to University College London, to read engineering, but he found he was not really as interested in the practical aspects of the work as he had at one time supposed, and he achieved only a Second Class degree.

On leaving the University he went to the National Physical Laboratory where he found himself working under Albert Campbell on the behaviour of iron under the influence of alternating magnetic fields. Through this work he became interested in magnetic detectors for radio waves, and he did a good deal of experimenting with radio apparatus at his own house. His first paper was published, jointly with Campbell, on the effect of Pupin loading coils on waves travelling along transmission lines.

In 1910 Eckersley left the N.P.L. and went to Cambridge to read mathematics. He entered Trinity College as an 'advanced student' and as such took only the more advanced specialist subjects in schedule B of the Tripos. He took the examination in June 1911 and was listed as having satisfied the examiners in these subjects; 'advanced students' were not awarded a Class. The regulations required him to wait until 1912 to take his B.A. degree. It was characteristic of him that he never went through the formality of converting this, later, to an M.A.

After taking the Tripos he spent some time in the Cavendish Laboratory continuing his previous investigations of the effect of high-frequency fields on magnetic substances. He did not make much progress, however, and he came to realize that his main interest was in theoretical, rather than experimental, work. After an unsuccessful attempt to get a Trinity Fellowship he therefore left Cambridge and joined the Egyptian Government Survey as an Inspector. One of his duties with the Survey was to make astronomical observations with the Reynolds 30-inch reflecting telescope.

On the outbreak of the 1914-18 war he took a commission in the Royal Engineers and was allotted duties associated with Wireless Intelligence in Egypt and Salonika. It was during this period that he developed that interest in the propagation of radio waves which was to remain with him for the rest of his life. The work with the Intelligence involved the accurate determination of the direction of arrival of radio waves from enemy transmitters, and Eckersley came to realize that the ordinary 'loop' direction finder did not always provide the correct answer. He concluded that a wave reflected downwards from the 'Heaviside' layer could give rise to direction-finding, or D/F, errors if the polarization of the wave was altered at reflexion from the layer, and he appreciated the essential features of what is now called 'polarization error' in direction-finding. A report which he sent to the War Office in 1916 probably formed the first scientific discussion of this subject.

Even when, as in the day time on the frequencies concerned, there was no wave reflected from the Heaviside layer, Eckersley found that there could be errors in the measured direction if the waves passed nearly tangentially across a coastal boundary between land and sea. He made a series of experiments to establish the reality of this 'coastal refraction', and tried to explain it in terms of Zenneck's theory of wave propagation over a partially conducting surface. Apparently, however, he did not notice that, although this theory gave approximately the measured magnitude of the effect, the sign was wrong. It is now realized that, although the Zenneck surface wave is a mathematically possible one, it is not, in fact, excited by a transmitter on the surface of the earth and the present theory of coastal refraction is given in terms of the more complete analysis of Sommerfeld.

When the war ended Eckersley joined Marconi's Wireless Telegraph Co. Ltd and remained with them till he retired in 1946. During that time he developed his interest in the theory of the propagation of radio waves and published several papers on the subject. In addition he led and inspired a small team of experimental workers, which just before the outbreak of the second world war numbered eight. Although Eckersley was always predominantly a theoretician he understood the importance of experimental work and often took part in observational work at all hours of the day or night. If a line of research was not going well it was his habit to say—'Let's try a damn fool experiment'—and was frequently rewarded with some new insight into the mechanism of radio-wave propagation.

Eckersley proved himself to be a wise technical adviser to the Company, and they came to value his scientifically disinterested views. It was largely as a result of his theoretical and experimental work that they were able so successfully to develop a great deal of their direction-finding equipment and their long-distance short-wave links.

The papers published while Eckersley was with the Marconi Company are concerned primarily with the propagation of waves through, and the scattering of waves from, the ionosphere; the origin of those naturally occurring radio waves of audible frequency called whistlers; and consideration of propagation by a full-wave theory rather than a ray theory. The papers on these subjects will be considered in turn.

Eckersley's interest in the ionosphere arose from his work on D/F errors, and his first published paper on the subject, written in 1921, is entitled 'The effect of the Heaviside layer on the apparent direction of electromagnetic waves.' It is one of the earliest papers to consider the effect of the ionosphere on waves of medium frequency reflected to medium distances. The earlier work of Eccles (1912) had been concerned with long waves transmitted to considerable distances. It was not until 1924 that Smith-Rose and Barfield published their papers on D/F errors, the year in which Appleton and Barnett, and Beit and Tuve, demonstrated the presence of down-coming waves more directly.

As Eckersley came to realize the importance of scattering from the ionosphere he also came to appreciate its importance in producing D/F errors. He therefore led his team to construct and use a spaced-loop direction finder which was free of polarization errors. Investigations with this led to some valuable information about scattering of waves reflected from the ionosphere at vertical incidence.

Eckersley's later papers on wave propagation through the ionosphere, published between 1930 and 1948, concerned somewhat unrelated topics such as 'The critical frequency in an ionized medium', 'Polarization of echoes from the Heaviside layer', 'Collision frequency of electrons', 'The existence of a biannual component in the F_2-layer', 'Critical frequency near $\tau=1$', 'Coupling of ordinary and extraordinary rays in the ionosphere', 'Triple splitting of ionospheric rays'.

The series of papers on scattering concern the phenomena which are now known as 'forward' scatter and 'back' scatter. They originated from observations made on commercial 'beamed' transmissions of morse code and of pictures. In the 'back' scatter observations signals were received, with an observable delay, either near the sender, or at points outside the 'beam', provided a directive receiver was orientated, not towards the sender, but towards that part of the ionosphere irradiated by the beam. In the observations on 'forward' scatter short pulses, transmitted over long paths, were often received two or more times, in rapid succession. Both types of observation were attributed to scattering from irregularities in the ionosphere. In two classical papers in the *Journal of the Institution of Electrical Engineers* Eckersley discussed the observational material and its theoretical explanation. He derived expressions for the angle, and frequency, dependence of the scattering from electron clouds of different shape, and discussed back-scatter both from these clouds direct, and also from the surface of the land or sea. These papers were published in 1929 and 1932, and, although at the present day they are known to be highly relevant to the important subjects of propagation by 'forward scatter', and of 'back-scatter sounding', they do not seem to have attracted any wide attention when they were written.

On the contrary, the group of papers concerning 'full wave theory' attracted considerable attention from the first. The title of the first of these papers, 'On the connexion between the ray theory of electric waves and dynamics' shows how Eckersley drew on his wide knowledge of physical theory to discuss wave-propagation in terms of other concepts. He had studied the early papers on wave-mechanics as they were published and had tried to apply the ideas to radio waves. For this purpose he developed what he called a 'phase-integral' method which he used to good effect in studying the propagation of waves round the surface of the earth. His later papers were published in collaboration with Millington, who first joined him in 1931.

One other paper which deserves especial mention is entitled 'A note on musical atmospheric disturbances' (1925). In it Eckersley discussed what are now called 'whistlers' or 'whistling atmospherics'. and showed that the succession of frequencies observed in them could arise by the dispersion of a radio-atmospheric as it travelled through the ionosphere. It was left to Storey, much later in 1953, to suggest the path which the disturbance followed.

When one re-reads Eckersley's papers in the light of present-day knowledge one is impressed by the fact that, although he often anticipated some later work, he is not usually given the credit for it. For example he was almost certainly the first to appreciate at all fully the implications of 'direction finding errors' in terms of waves reflected, with altered polarization, from the ionosphere, and his early papers on the ionospheric propagation of short waves contain some valuable suggestions. The papers on scattering were far in advance of their time, indeed much of the matter in them was rediscovered after the war.

It is interesting to speculate about the reason for this apparent neglect of some of his pioneer work. The reason can probably be found in Eckersley's own method of writing. He was a very original worker who thought about wave-propagation in his own way and never troubled to relate his approach to other people's. His papers are often difficult to follow, and generally make use of a nomenclature peculiar to themselves. This is particularly noticeable in the papers on the magneto-ionic theory, in which Eckersley continued to use his own nomenclature to the end, long after the large majority of workers had adopted Appleton's nomenclature. An example is in the title of his paper 'Critical frequency near $\tau=1$', published in 1948, which is without meaning except to those who know Eckersley's own nomenclature for the magneto-ionic theory.

Not only, however, did Eckersley fail to attract proper attention to his work by failing to relate it to that of others, he also tried to include too much in a single paper. Some of his most important papers on short wave propagation in the *Journal of the I.E.E.* give the impression that he was trying in one paper to explain all that was known on the subject. It is not always surprising that those who read these papers at the time could not properly assess them, whereas those who read them now, with the advantage of knowing which of the many ideas have proved to be correct, realize their very great importance.

In spite of the fact that Eckersley's work was not all so well appreciated as it might have been, his ability as an investigator was widely recognized. He became a Fellow of the Royal Society in 1938 and was awarded the Faraday Medal of the Institution of Electrical Engineers in 1951. For each of his major papers in the *Journal of the I.E.E.* he received a Premium.

Eckersley's wide knowledge of radio-wave propagation made him a valued member of U.R.S.I. and of C.C.I.R. and he attended their Assemblies regularly until his health broke down. When he retired in 1946 he was already suffering from multiple sclerosis, and, although he continued to do theoretical work at home the disease pursued its inevitable course and in his later years he was almost completely helpless. During this time Millington gave him considerable help and encouragement by discussing his theories and ideas with him, and he was supported throughout by the loving care of his wife, who not only wrote out most of his work and his publications for him, but also attended to his every need. He finally died on 15 February 1959, of pneumonia contracted during an epidemic of influenza.

J. A. Ratcliffe

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Alexander Gibb

ALEXANDER GIBB

1872-1958

ALEXANDER GIBB was one of those fortunate men in whom family tradition, natural gifts and personal bent appear to have agreed from the beginning as to the choice of a career. His great-great-grandfather, William Gibb of Gartcows, belonged to that first great generation of civil engineers which included James Brindley and John Smeaton. His great-grandfather, John, was one of Thomas Telford's most trusted deputies and a founder member of the Institution of Civil Engineers. His grandfather, Alexander, was a pupil of Telford and associate of Robert Stevenson. His father founded the contracting firm of Easton Gibb & Son and was engaged on railway construction in Scotland when the subject of this memoir was born at Broughty Ferry on 12 February 1872, the fourth of eleven children.

Even at his preparatory school Alexander showed signs of unusual capabilities. Gardening was encouraged; and young Gibb's operations, assisted by a system of forced labour among his juniors, were such as to enable him to sell gravel to his schoolmates for making paths, and later to supply water for irrigation. Ultimately they provided a means of egress outside the school boundaries.

In the classroom his inclinations were towards drawing, poetry and the classics; which, though perhaps unexpected in a young engineer, all represented permanent aspects of a many-sided character. He went on to Rugby, where (though according to his own account he did 'just enough work to keep out of trouble') he excelled in electricity and again showed his talent for drawing. It was, however, the holidays spent among the Yorkshire dales —the family home was now at Beamsley Hall, near Skipton: Easton Gibb was constructing a reservoir in Wharfedale—that perhaps left the deeper impression on his mind during this period.

There was no doubt that Alexander Gibb was going to be an engineer. For a time, influenced perhaps by his success at school, he favoured the electrical branch: but, almost inevitably, the final decision was that he should follow the road of his forebears. He went to University College, London. Alexander Kennedy had left the college by that time, having created a fine engineering laboratory, to found the firm of Kennedy & Donkin, electrical engineering consultants, with which Gibb was to have close associations later. His teachers included Thomas Hudson Beare, later Regius Professor of Engineering at Edinburgh, and Vernon Harcourt whose books on tides and hydraulics are well known. After a year Gibb went as articled pupil into the office of Sir John Wolfe Barry and Henry Marc Brunel.

Wolfe Barry was acknowledged as one of the two leading civil engineers of his day. His powerful personality, wide activities and kindly interest in the younger generation, with Brunel's originality and whimsical humour, together created an atmosphere in which embryonic talent flourished remarkably. Among the pupils in the Delahaye Street office were a number who were to reach the top rank in various walks of life besides engineering; and Gibb made many lifelong friends, including two—Rustat Blake and Leopold Savile—who in time became his partners. One of his companions, Robert Dana the naval architect, wrote these words some fifty years later:

'Alex was full of fun and frolic, but this did not prevent his being one of the most industrious. By his quick intelligence and resourcefulness he soon made his mark. Nothing was too much trouble, or came amiss to him, and although his handwriting was hardly suited to the copperplate lettering expected of the conscientious draughtsman, yet even this disability was overcome by patient application. Again, his knowledge of mathematics and science was slender, but he had the gift of making the most of such knowledge as he possessed, an unerring instinct for its practical application to engineering problems.'

After two years in the office Gibb was given outside experience, first with Formans and McCall of Glasgow, engineering consultants to the Caledonian Railway to which Wolfe Barry also was consultant, and later on the new dock at Barry in South Wales. At the end of his five years' pupilage he stayed on with Wolfe Barry for another two years, turning down several tempting and remunerative offers in favour of an appointment as Barry's Resident Engineer on the extension of the Metropolitan Railway between Whitechapel and Bow at a salary of £300 a year.

Easton Gibb had obtained one of Barry's most important contracts—the King Edward the Seventh bridge at Kew. For two years he looked after the work himself; then his health began to fail; and the business needed the attention of a younger man. Alexander meanwhile had met Norah, daughter of Fleet-Surgeon Lowry Monteith, R.N., and wanted to marry, which was impossible on his present income. There was only one thing to do. Barry opposed the move on the ground that from consulting engineer to contractor was a downward step (though his real reasons probably went much deeper). But Gibb had made up his mind.

He worked as a contractor for sixteen years, completing the Kew bridge and a succession of other works, and moving his home from place to place as necessary to be near the scene of his labours. He never kept business and family life in watertight compartments. Hospitality was something of a passion with him, in which he was enthusiastically supported by his wife; and he treasured a collection of visitors' books in which the names of old friends mingled with those of people prominent in many walks of life. Wherever his duties took him he managed to find a house combining ample facilities for entertainment with opportunities for at least some of the country sports—hunting, shooting and fishing—which he loved, and into which he initiated his three sons as soon as each was old enough.

His last and greatest contract was for construction of the naval dockyard as Rosyth. Owing to administrative delays and various difficulties it appeared at one time impossible that the dockyard could be completed in time to be of substantial use to the Fleet during the most vital period of the 1914-18 war. It was; and this was due to Alexander Gibb who, through his friend Winston Churchill, put forward an accelerated construction programme and carried it through with extraordinary courage and persistence. One of those who was closely associated with him at this period—probably the most critical in his career—has described it in the following words:

'In the course of forty years' subsequent experience I have never elsewhere encountered anything to compare with the desperate character of the engineering struggle which took place at Rosyth during these years, a struggle which Mr Gibb had to make against time and harsh natural conditions on the one hand and against an unsympathetic lack of official understanding on the other.'

During this time, as always, Gibb was busy with many other activities. He was given charge of submarine nets around the Scottish coast; worked with Lord Elgin, Lord Lieutenant of Fife, on anti-invasion plans for the county; became a prominent figure in Scottish Freemasonry; presided, as a last-moment deputy, at a Synod of a Scottish Church; entertained, as was now necessary, on a larger scale than before in the big house which he had taken near Perth; made friends with naval men of all ranks; organized shooting parties and practical jokes.

In 1916 he accepted an appointment under Sir Eric Geddes, with whom he was to be closely associated for some years, as Chief Engineer, Ports Construction, to the British armies in France. His responsibility was to prepare for reconstruction of ports and railway junctions in Belgium which the Germans were expected to demolish in their retreat before the Allied offensive planned for the spring of 1917. This never materialized; and when, in July 1918, the Allies did attack, the enemy had no time to do more than superficial damage, nor were the Belgian ports essential for military operations. Gibb, however, was instructed for the sake of Belgian goodwill to put them in order, removing the effects of neglect and of the famous British raids; and for this King Albert made him a Commander of the Order of the Crown of Belgium.

His next appointment—virtually a new office created for him by Geddes who was now First Lord—was Civil Engineer-in-Chief, Admiralty. The work consisted mainly of organization and administration and was not congenial to Gibb; but he did it with his usual thoroughness, with the result that the constructional output of his Department increased substantially at a critical period of the war. At the same time he was working on a scheme to counter the U-boat menace. This, when he first proposed it in 1917, had been turned down as wildly impractical; but a year later the situation was so desperate that the authorities were willing to give it a trial. Gibb's idea was to construct floating towers which could be towed out and sunk in a line across the Channel, forming gun emplacements and bases for mines and nets. In its final form the project was married with another for submarine location by galvanometer, conceived by William Bragg and an electrical engineer, Colonel William McLellan, who later became Gibb's close friend and associate. They reckoned that their device would have an effective range of two or three miles, so that one placed on each of Gibb's towers would form a continuous screen from coast to coast. Construction of the famous 'Mystery Towers' was put in hand at Shoreham. Several of them were nearly ready, and the scheme would have been in action in another three months, when the Armistice was signed. Gibb had the melancholy satisfaction of seeing one of his brain children erected at Shoreham, where trippers paid a shilling to climb to the top of it. Another, however, was towed out and satisfactorily sunk off Portsmouth, thus proving at least that the operation was practicable in spite of many expert opinions to the contrary. And the principle was applied in the second world war to provide anti-aircraft gun emplacements in the Thames and Mersey.

Gibb was created a C.B. for his services to the armies in France and a K.B.E. on appointment to the Admiralty. His work there was recognized by advancement to G.B.E.

It was once more Geddes, now first Minister of Transport, who appointed Gibb his Director-General of Civil Engineering in 1919. He had to create a new Department in a new Ministry while dealing with a variety of problems connected with communications in the British Isles. Among the projects which came under his review were the Channel Tunnel and the Severn Barrage. The latter attracted Gibb, who was already becoming vitally interested in hydro-electric power. With the help of some of his old associates he prepared proposals which departed in several respects from those which had been put forward previously, including road and rail bridges, and he dealt with the chief limitation of all tidal schemes, intermittent power supply, by the use of pumped storage—a device which is only now beginning to receive the attention it deserves. He did this work at the special behest of Geddes. When the plan was made public he was suspected by other engineers, who had been working independently on the same problem, of poaching. They could not know his reasons and he could not defend himself. His proposals were well received in many quarters; particularly in South Wales, which stood to gain most from such an integrated scheme, and which would certainly have benefited from it during the depression that followed not many years later. There is no need to go into the reasons why nothing was done about it. Many years after—in 1944, in a lecture to the Institution of Chemical Engineers—Gibb said:

'The Severn Barrage remains as a scheme in the lap of the gods and awaiting the day when those empowered to decide are at last satisfied that the scales are so weighted in its favour as to justify a real start being made on this great pioneer work. There is no doubt in my mind but that that day will come.'

Gibb was now beginning to feel that he had done his duty sufficiently in public offices involving much work of a kind that was uncongenial to him. He had come into close contact with two electrical engineers—Colonel William McLellan already mentioned, and Dr Charles Merz, the leading authority of his day on electricity distribution and one of the prime initiators of the Grid—who shared many of his ideas and encouraged him in the scheme that was forming in his mind; indeed, they had similar plans themselves, and the firms of consulting engineers which he and they founded were to carry on throughout the years a collaboration which had already begun on a personal level. To them, as to all his many friends, he was always the first to acknowledge his indebtedness.

In 1921 Gibb set up in a two-room office in Victoria Street as a consulting civil engineer. Next year the firm of Sir Alexander Gibb & Partners began its existence in Queen Anne's Lodge, Westminster, a historic eighteenth-century house that provided just the atmosphere he thought appropriate for the kind of firm he envisaged—an organization which would be one of the largest in this country. His first partner was Rustat Blake, his contemporary as a pupil under Wolfe Barry and until 1922 a partner in Barry's firm. The next, John Ferguson, came a few months later from Formans & McCall of Glasgow, old friends who also provided—very willingly because it was to help Gibb—a number of recruits to the staff at Queen Anne's Lodge.

Gibb was now fifty. He had an assured reputation. He was making what was virtually a new start in life. Ten years of anxiety and struggle were ahead before the firm was on its feet; and Gibb used his money as a sheet-anchor, carrying the whole financial burden himself and asking nothing in this respect from his partners. Besides attending to the great variety of consulting work in the office, he travelled widely, chiefly with the object of getting to know and getting known. In seventeen years he covered 280000 miles and visited sixty countries. Meanwhile he held a variety of public offices, of which his Presidency of the Institution of Civil Engineers was the one which he himself valued above all others; and he continued to follow actively a wide range of private interests. To say that he was the author of the standard biography of Thomas Telford whom he greatly admired, honorary president of the Burns Federation, a prominent Freemason, a member of the Royal Fine Arts Commission, an Honorary Doctor of Law of Edinburgh University, a member of the Queen's Bodyguard for Scotland (Royal Company of Archers) and an active supporter of charitable causes that appealed to him, is to give only instances of the demands on his time and energy, which he always gave unsparingly.

His election, in 1936, to Fellowship of the Royal Society was an honour which particularly gratified Gibb. It was the greatest satisfaction to be joined with Telford and that small select band from his profession whose work was judged to merit such high scientific recognition.

From 1922 onwards Gibb's professional activities became inseparable from those of his firm, and he would have been the last to claim an undue share of the personal credit for anything they achieved. He and his partners always worked closely together. They were joined in 1932 at about the date of Ferguson's retirement by Hugh Beaver (later Sir Hugh Beaver) and by Sir Leopold Savile, another Wolfe Barry pupil and Gibb's successor as Civil Engineer-in-Chief, Admiralty, and later by others, each of whom brought particular qualities of character and experience. He had no belief in the practice of selling partnerships in his firm. Partners were selected either from his tried and trusted friends who had grown up with him in engineering or else from the members of his staff who had the necessary experience, ability, personality and, not least, loyalty that he expected and invariably received. Then there was the staff, continually increasing in numbers, who bore the brunt of detail work. To them, as to his other advisers and many friends, Gibb always acknowledged his indebtedness. Without attempting, however, to assess precise degrees of responsibility, it can be said unequivocally that he was both the initiator and the mainspring of inspiration. One of his oldest and closest associates has referred to his 'uncanny knack of choosing the right people to work with him'. Having chosen them, he would support them through thick and thin, right or wrong. As a result he got the best out of them.

With this proviso some instances of the firm's work may be mentioned: indeed this memoir would be incomplete without them since, collectively, they represent by far the major achievement of Gibb's life. Their scope was considerable even from the beginning. A list of the more important works in hand during the remaining seven months of 1922 includes:

Design and supervision of the aquarium for the Zoological Society of London.

Supervision of hydro-electric development, construction of town site and paper mills at Corner Brook, Newfoundland, for the Newfoundland Power & Paper Co. Ltd.

Advice on Itabira (Brazil) iron ore project—mining development, railway transportation and shipping facilities.

Design and supervision of dock and harbour extensions for the Ipswich Dock Commission.

Advice to the Southern Punjab Railway Co. Ltd.

Advice to the Darjeeling-Himalayan Railways Co. Ltd.

First designs, the Barking power station, for the County of London Electric Supply Co. Ltd.

The firm were appointed civil engineering consultants for the Barking power station through the good offices of Gibb's old friend Charles Merz, who was electrical engineering consultant to the company. It occupied them for many years (and in fact they are still engaged on certain minor extensions at the time of writing this memoir—December 1958). The original scheme provided for the construction in four stages with an ultimate capacity of over

500000 kilowatts, constituting the largest thermal station in Europe at that time; though far exceeded by some of the latest projects of the Central Electricity Generating Board, on several of which Sir Alexander Gibb & Partners and Merz and McLellan are working in collaboration.

Hydro-electric power was one of Gibb's main interests quite apart from the fact that in the design and construction of dams, aqueducts, tunnels and other works for the storage and delivery of water it offers far greater scope, in relation to output, for the civil engineer than thermal power. He was one of those who saw and proclaimed its economic promise at a time when scheme after scheme, prepared with infinite pains and at great cost, was being thrown out of Parliament with scarcely a hearing. The firm reviewed the civil engineering aspects of part of the ill-fated West Highland scheme; some of the staff still remember the survey work which they carried out among Scotland's most romantic scenery during the summer of 1928, in continual rain and pursued by relentless 'clegs' as horse flies are called north of the Border; and Gibb himself gave evidence before the House of Lords Committee sitting on the Bill. But it was in vain.

The one exception, during these inter-war years, to the general rule of implacable opposition to hydro-electric projects was the Galloway scheme, the first major scheme designed to operate on a low load factor for peak load demand through the newly instituted Grid, and the beginning of a new era in electricity supply. On this once more the firm were in collaboration with Merz and McLellan. Merz had done as much as anyone to bring the Grid into being. McLellan worked with Gibb on the various sites in the West Lowland valleys until he died one night as he was walking alone across the marshes to his home—the effect of overwork on a constitution impaired by ill health. So Gibb lost one of his best friends.

The Galloway scheme was completed in October 1936, and within a few hours the Central Electricity Board was making use, through the Grid, of the full available output of its five stations. It is strange, considering its immense success, that it was the last major hydro-electric scheme to get through Parliament for sixteen years. But common sense, supported by economic necessity, prevailed in the end. The Cooper committee was instituted to investigate the whole matter. As a result of its recommendations the North of Scotland Hydro-Electric Board was set up in 1943; and Gibb lived to see a crop of schemes under construction in Scotland, of which his own firm were responsible under the Board for two.

Docks and harbours, another important development of civil engineering, can best be represented here by the story of Rangoon. It is an interesting story for several reasons, not the least being the fact that the outcome of several years' intensive work by the firm was a recommendation that nothing should be done to stop the apparently progressive blocking of Burma's chief port by siltation. Also, the essential focus of this work was a large hydraulic model—an early example of its kind and a pioneer effort in certain respects. It was also the subject of a paper delivered to the Institution of Civil

Engineers. The technical aspects are summarized in an Appendix to this memoir. Here, it is only possible to tell the story briefly.

About 1910 it was observed that a silt bar about 7 miles wide was building up across the entrance to the Rangoon river, which forms the navigable channel to the port of Rangoon. This was known as the Outer Bar. By 1931 the ruling depth was no more than 12 feet at low water and it was still decreasing. One of the chief ports of the East, and the main channel of Burma's export and import trade, was threatened with strangulation and no expense would be spared to save it. The Port Commissioners consulted Gibb in 1929. He had his observers on the spot and twice visited the site in person.

The complex causes of the growth of the Outer Bar, the future course of events, and the effects of any proposed remedial measures were an excellent subject for model experiments. In his days at the Ministry of Transport Gibb had become familiar with the tidal model constructed by Professor A. H. Gibson to study the effects of the proposed Severn Barrage. He decided to apply this method to the present problem although there were much greater difficulties in simulating the natural forces involved and the data available were less complete, than in the former instance. The model was constructed in a basement belonging to University College London, and was in operation for some three years. Meanwhile the Outer Bar was still growing and the Rangoon authorities were, naturally, getting a little impatient. They wanted action. Many people, including some engineers, openly derided the model.

The firm's report was submitted in 1935. Its main conclusion, based on the evidence of the model, was that the growth of the Outer Bar was about to cease, and that thereafter it would gradually disappear as spontaneously as it had come. The port authorities, who were prepared for substantial expenditure on any measures that might conceivably be effective, must have had some difficulty in accepting this conclusion. However, they did accept it. In 1936 it was observed that the depth over the Outer Bar had increased very slightly. In 1948, although dredging ceased completely while Burma was in Japanese hands, it was 18 feet at low water. (See Appendix.)

These are only a few examples of the consulting work on which Gibb and his firm were engaged during the inter-war years. There was also wharf construction at Rangoon. A few years earlier Gibb paid three visits to Colombia and had a resident representative (Colonel Warren, another old Wolfe Barry pupil) there for some time. The chief problem here was another port, Barranquilla, threatened by silt; though in this case the solution was the construction of two long moles. In 1931 he embarked on a whirlwind tour of ports on the eastern and western coasts of Canada. The resulting report (which was published as a Blue Book) shows that he was quite capable, on occasion, of adapting his mind to problems not essentially of an engineering nature: its main recommendations were concerned with administrative reforms. It was accepted, and the results have been invaluable to Canada's trade, although Gibb said himself that Canadians remembered him chiefly as the man who taught them the proper way to sing 'Auld Lang Syne'. He was in India four times (once as part of a world tour including Tokyo where he addressed the World Engineering Congress). At home, besides Barking and Galloway, the firm were responsible for major repairs to Telford's Menai suspension bridge, and for the design and supervision of construction of the swing bridge over the Forth at Kincardine. Their collaboration, as civil engineering consultants, with Sir Giles Gilbert Scott in creating the great Guinness brewery at Park Royal was to lead in due time to a far-reaching expansion of industrial work.

Gibb himself had personal professional responsibilities in addition to the firm's consulting work. He was in demand among promoters of engineering schemes as an expert witness. Sir Lynden Macassey, K.B.E., who met him frequently in this connexion, wrote of him on his death:

'In long experience of the great engineers in the Private Bill Committee Rooms he was undoubtedly the greatest within my recollection—certainly one of the greatest engineering witnesses. He had the gift of most lucid exposition of the facts of the case as he saw them, and invariably explained them with an attractive persuasiveness which soon established a hold on his tribunal.'

There was a period in 1935 when he was president of six societies at once. In 1936-7 he was president of the Institution of Civil Engineers (of which he had been a corporate member for over sixty years when he died). During this time he was continually pressing for the reforms that were nearest to his heart, particularly for co-ordination of effort and co-ordination of scientific knowledge. His numerous addresses are listed in an Appendix to this memoir.

It is no exaggeration to say that he was putting himself under considerable strain. Late one night, in his flat in Queen Anne's Lodge at the end of a strenuous day, he tripped and fell, damaging an eye, which was removed the same night by Sir Stewart Duke-Elder. When he interviewed the Italian specialists who were to make him an artificial eye he instructed them to 'put a twinkle in it'. The accident happened in 1932 when he was sixty. He did not relax but, on the contrary, the years that followed were probably the busiest of his life. In 1937, after his return from Canada, he had to go abroad to consult a German specialist. The next year business took him to Colombia and Venezuela, and it was hoped that the long voyage would put him right. Unfortunately in the autumn of 1938 he had a further relapse and after some weeks in a nursing home he spent five months recuperating. He was persuaded to take another sea trip for health reasons alone during the summer of 1939 but collapsed on the voyage and was hurried back to England.

The autumn of 1939, which plunged the firm into a welter of urgent war work, was for Gibb the beginning of a long convalescence during which he received news of three deaths—those of the wife who had been his constant companion and loyal supporter for so many years; of his fellow pupil and partner Rustat Blake; and of Charles Merz, who was killed with his whole family by a bomb. It was not until 1942 that he returned to take up his residence within reach of London. He was hoping for appointment to some high office for which his achievements qualified him. But three years is a long time to be out of the running; and in any case it is doubtful whether he now had the strength for great responsibilities.

He took disappointment cheerfully, showing a degree of patience that astonished those who had known him in earlier days. From his small house at Hartley Wintney in North Hampshire he kept a close eye on everything that the firm was doing, paying regular visits to Queen Anne's Lodge, to the site of the vast underground aeroplane engine factory in the old Bath stone quarries at Corsham, and to the house at Rotherwick where a detachment of the staff were working full pressure on plans for the Captain Cook Naval graving dock at Sydney. These visits were no mere formality. Referring to this time and the early post-war years, one of his partners wrote:

'Even then his personality made an impact on the staff. I always felt that it was possible, on entering Queen Anne's Lodge, to sense whether or not Sir Alexander was in the office by the general effect that his presence created. Just as in a hive, when the queen is busily pursuing her duties, there is a deep hum of contented activity from the worker bees all fully occupied on their varied tasks, so Sir Alexander could create an increase in the efficient working of the staff throughout the office.'

Gibb's eldest son, Alistair, who was commanding a regiment in Italy at the time, was released towards the end of the war as a matter of urgency to help with the work. Shortly afterwards he became head of the firm. Alistair resembled his father remarkably in one respect: he took bold decisions which caused many older heads—including perhaps his father's—to shake, but which time proved to be wise. Sir Alexander lived long enough to witness that proof and it was the last great satisfaction of his life. The last great tragedy was Alistair's death after an accident on the polo field in 1955. The consolation was that Alistair's youngest brother, Michael, was also with the firm and had by that time worked his way up to a partnership.

This is no place to trace the firm's fortunes further. And yet there is no other way to complete the story of Alexander Gibb's life. He was one of those who present insuperable problems to a biographer, not so much because their characters and activities are many-sided as because there is nowhere to write *finis*. Plenty of works, on the largest possible scale, stand as records of his achievement. They are finished; but his influence, always extraordinary, is still active. Only those who have known him and worked with him or under him can realize the extent to which this is true.

Let his favourite Burns provide his epitaph:

> 'Few hearts like his, with virtue warm'd,
> Few heads with knowledge so inform'd.
> If there's another world, he lives in bliss,
> If there is none, he made the best of this.'

G. P. Harrison
A. J. S. Pippard

APPENDIX. RANGOON MODEL

The Rangoon model constructed during 1931 was the largest and one of the most complex which had been developed in Great Britain up to that time. It was necessary automatically to reproduce in the model a wide variety of variables, among these being the cycle of spring and neap tides with a pronounced diurnal variation, also automatically to produce dry weather flow and monsoon floods in the Rangoon River at the appropriate seasonal periods together with the necessary heavy silt content during the floods, which material had to be carried along by the fresh river water and deposited precisely and at an accelerated rate at the deltaic river entrance and particularly over the area occupied by the Outer Bar. The effect of monsoon winds on the currents in the Gulf of Martaban and the littoral drift of silt in the gulf towards the river entrance had also to be reproduced.

The model measured 40 ft. by 60 ft. The scale in plan was nine inches to one sea mile, with a vertical exaggeration of 42 times, or one inch representing 16 ft. The tidal movement was reproduced by means of displacement plungers and with the scales adopted the time scale became accelerated by 583 times so that one year was represented by 15 hours' operation of the model.

At the start of the tests, the river and sea bed were modelled to the conditions prevailing in 1875 and the model was run through continuously until 1932. The results achieved showed sufficient similarity to the actual conditions existing at that date as to justify proceeding with predictive runs into the future as far as 1982 in one case, and up to the year 2000 in the final study.

These showed that the deposition of silt in the Outer Bar which was interfering with navigation to the Port of Rangoon would reach its peak, giving about 12 ft. depth of water only, in 1936, some four years after the model experiments. From that date, there would be a gradual improvement in depth to 20 ft. or more. It was on the basis of these model results that Sir Alexander Gibb advised the Commissioners for the Port of Rangoon that no expensive remedial works would be required and that the river itself would carry out the necessary improvements. The model results have been confirmed since 1932. The improvement began in 1936, as predicted, and has continued since. At the present time, 1959, the depth that has been maintained over the Outer Bar is adequate for the navigation requirements of the Port of Rangoon.

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CLAUDE DIXON GIBB

1888-1959

UNLIKE most of the men whose lives and work are recorded in these biographical memoirs Claude Gibb was not primarily a scientist; he was an engineer, a business man and an administrator.

CLAUDE DIXON GIBB was born in Adelaide on 29 June 1888; throughout his life he had a trace of Australian accent and his boundless energy, his self-confidence and his amiable pugnacity were typically Australian. He loved speed both in business and in movement; in his own notes for the Royal Society he thought it worthwhile to record that he was riding horses at the age of three, motor-cycling at the age of fourteen, motoring at sixteen and flying at nineteen!

Gibb went to Alberton School until he was eleven years old, when he won a scholarship to the Lefevre High School. From there another scholarship took him to the South Australian School of Mines where he studied mechanical and electrical engineering from 1914 to 1917. While working there he got workshop experience on the outside staff of Gibb & Miller, a firm of constructional engineers in which his elder brother was a partner.

He joined the Adelaide Cement Company as an electrician and stayed for nine months until in 1917 he was old enough to join the Australian Flying Corps as an air mechanic. He became a pilot and served in France. While on leave from the A.F.C. he visited friends and relations in England and it was during one of these visits that he first met Miss Margaret Harris of Totnes whom he married in 1925 when he returned to England.

After the war Gibb went back to Australia and obtained a post at Adelaide University as Senior Research Assistant on the Staff of Sir Robert Chapman, K.C.M.G., who was Professor of Engineering at the University. He took his Degree of Bachelor of Engineering and obtained the Diploma in Applied Science in 1923; he did research on the hydration of cements, on heat treatment of alloy steels and on the physical properties of Australian hard woods.

At that time the possibility of getting broad engineering experience in Australia was limited and Gibb decided that he should have one or two years' experience in England before settling down to an engineering career in Australia. He was put in touch with Messrs C. A. Parsons and it was arranged that he should be taken on at their works. Before he left he submitted a thesis to the University of Adelaide which won for him an Angas Engineering Research Scholarship, given to encourage Australian engineers to get experience abroad.

Gibb arrived at Parsons's Heaton Works on 3 March 1924 and was graded as a student apprentice receiving a wage of £2 14s. a week; he started work in the Erecting Shops. After about a year he was transferred to the Drawing Office and from there to the outside erection staff in the London Area. It was on this job that he made his first personal contact with Sir Charles Parsons. Gibb was working at the Barking 'A' Power Station when, on 14 December 1925, one of the machines was being started up after the station had been standing cold over the weekend. Severe weather had frozen the drain discharge pipes and this caused a water hammer which blew the cover off the steam chest. The Company's resident engineer was ill at the time and Gibb was the senior man on the erection staff when the accident occurred. The clear account which he gave to Sir Charles Parsons created a sufficiently favourable impression on that rather frightening man for him to have Gibb brought back to Heaton where he was made Manager of the Steam Test House. From there he went to the Design and Drawing Offices with a general commission to overhaul the organization and improve the procedure for investigating troubles.

Sir Charles Parsons kept in touch with Gibb's work and in June 1929 made him a Director and Chief Engineer. The appointment appears to have been made without prior consultation with the other Directors; Gibb's own story was that, having been called to Sir Charles Parsons's office to discuss an incident, Sir Charles said quite suddenly and unexpectedly, 'I am making you a Director, come and have a cup of tea'.

Parsons died in 1931 and in the subsequent reorganization Gibb, as Chief Engineer and Technical Director on the mechanical side, became largely responsible for running the works. It was at this time that he first came in contact with Engineer Vice-Admiral Sir Harold Brown who was then Engineer-in-Chief of the Fleet; naval turbine practice was lagging behind land practice and Sir Harold Brown's consultations with C. A. Parsons were aimed at rectifying this.

In 1932 Gibb made a tour of the United States of America and Canada and in 1936 he spent some months in Australia where he succeeded in obtaining the order for the 50 MW turbo-generators for the Bunnerong Power Station of the Sydney County Council in the face of fierce competition. He was made General Manager of Parsons in 1938.

The work which Gibb had done in connexion with Admiralty turbines had made a deep impression on Sir Harold Brown who, on retiring from the Navy, became Director-General of Munitions Production at the War Office and subsequently at the Ministry of Supply. When the urgent need for reorganizing the supply organization arose in 1940 the Admiral asked Gibb to join his staff at the Ministry of Supply. Of that appointment Brown writes 'It was natural that when in October 1940 we were called upon to replace the loss of equipment in France, to speed up the existing programme and in many cases to double it (with a note from the P.M. "Pray inform me when this will be done"), and it became urgently necessary to bring in assistance on the engineering side of the supply organization that I should appeal to Gibb. The response was immediate for without any fuss as to conditions or what was required of him and indeed with no regard for his personal interests, he arrived the very next day eager to get down to the job.' He first worked as personal assistant to Sir Harold Brown and shortly after was made Director of Weapon Production and later Director-General of Weapons and Instruments Production. Of his work there Sir Harold writes 'He thus became a leading member of the team and with his outstanding engineering knowledge and experience, immense drive and energy, infused fresh life into the organization and helped to produce results which without him would have been almost impossible. This he did by bringing a fresh mind on to what was for him a new field of engineering.'

Gibb of course knew little about guns, it was his drive, his organizing ability and his common sense which enabled him to do outstanding work without offending the susceptibilities of the older hands. When, in an argument, he was teased that he really knew nothing about guns he answered that so far as he was concerned a gun was the simplest form of internal combustion engine that he had ever dealt with. When, after the Battle of Alamein, he received a telegram from the Prime Minister acknowledging the contribution to success which had been made by the 25 pounder gun and saying 'It is you who had made this gun available in these great quantities', Gibb's comment to his friends was that a 25 pounder gun every 25 yards on a six mile front required only a month's output.

In 1943 Gibb left his post as Director-General of Weapons Production to become Director-General of Armoured Fighting Vehicles and later Chairman of the Tank Board. At that time tanks were a black spot in armament production. Design had been dispersed in the offices of a number of industrial manufacturers and co-ordination had not been effective. Gibb immediately took the decision that his department would take full responsibilities for design and brought all of his own energy, engineering experience and practical common sense to bear on the problem. 'Trouble with gears?' he said in discussion with his experts. 'Cut out this pansy involute stuff and get back to stubby cycloidals.' He realized that it was no use to have new designs tested only on the proving grounds where they were put through exacting tests by expert drivers; he arranged for prototypes to be sent also to the Tank Training Depots where, as he said, the incompetent driver had the opportunity of doing his worst to them. The result of his work was the 'Centurion' which was probably the outstanding tank of the war. Besides this, under his direction, the Tank Board produced the stabilized gun, the 17 pounder as fitted to the Sherman at a critical time, and all of the special tank developments for wading and for infantry support.

At the end of the war Gibb was offered posts with other large engineering concerns but chose to return to Parsons where he became Chairman and Managing Director in September 1945. To those who knew him it was natural that he should return to Newcastle. His love for and his pride in the

Parsons organization were unbounded and he wished for nothing more than to make them outstanding in their field. In this he succeeded. Towards the end of the war he had worked out a scheme for equipping the heavy machine shops at Heaton with new machine tools of modern design and employing up-to-date cutting speeds. He had surmounted the difficulties of licences and priorities and had got the machines ordered for manufacture immediately the war came to an end. It was this action which enabled Parsons to expand their output to meet the heavy post-war demands of the Electricity Authorities with such success. Further expansions of the factory followed under his direction, light machine tools were installed in the shop which had been built for the production of six pounder guns, the foundry was reconstructed and extended to enable it to deal with castings of the size needed in post-war practice, and new shops for building the cores and fitting the windings in generator stators of the size then envisaged were built. In 1955 the development of turbo-alternator sizes made further extensions necessary and he had a new bay with a 200-ton crane added to the erecting shops and new machine shops constructed on land adjoining the original Parsons factory.

Gibb realized the importance of research in the turbo-alternator and transformer field and in 1951 he built a large new block to house part of the Research Department, the Design Office and the administrative staff, and followed this with another even larger Research Laboratory devoted mainly to the problems of nuclear power which was completed in 1959, shortly after his death.

His business interests were not confined to Messrs C. A. Parsons. He was Chairman of Messrs Grubb, Parsons & Company and took a close technical interest in the highly specialized work which went on in their shops. He was also Chairman of Reyrolles.

It was in January 1944 whilst still engaged on war production that Sir Claude joined the Board of this Company. The link between Reyrolle and Parsons had by then been in existence for many years and his energy, drive, and forcefulness had long been apparent to the Reyrolle Directors who, in anticipation of the return to peacetime production and its many difficulties, invited him to join them. His wise counsel and his appreciation of the requirements of industry in the post-war years were invaluable in the vast expansion which followed the end of the war. In spite of the fact that at Heaton he had more than a full-time job he always had the time and took the trouble to follow this expansion in great detail.

In 1945 Sir Claude was appointed Deputy Chairman of the Reyrolle Board, and in 1949 became Chairman; he retained this office until October 1958. It was during the period of his Chairmanship that the Reyrolle organization expanded at a greater rate than ever before. He ensured that a large part of the profits was ploughed back into the business to finance the programme of new buildings and research facilities. The most important of these were the high-voltage testing station and network analyzer; the light engineering shop for the manufacture of relays; extensions to the high voltage assembly shop and new research laboratories and tool room.

For many years Gibb represented the Reyrolle interests on the Boards of Directors of The Bushing Co., Pyrotenax Ltd, and Savage & Parsons.

One of the qualities most admired by all employees in the factories for which he was responsible was the pleasure which he took in meeting and mixing with people on the shop floor; he did this as it can only be done by a man who has worked on the shop floor himself. His understanding of the hopes, the fears and the ambitions of work-people, as well as his realization of national needs made him deeply interested in apprentice training. He discarded the practice of filling the Apprentice School with old machine tools and equipped it with modern machines which were housed in part of the Tool Room so that the apprentices came under the influence of the most highly skilled mechanics.

It was typical of Gibb that with all the load of work arising from the post-war reorganization of Messrs C. A. Parsons and Reyrolles and with the problems of meeting the post-war demands of the electricity supply industry and re-building the export trade of the heavy electrical industry he was one of the few men who found time, in the early post-war years, to realize the importance of the developments which were taking place in the nuclear field. In 1946 his firm undertook the design and manufacture of the d.c. electromagnet and vacuum chamber for the Harwell 800 ton cyclotron and he collaborated with Risley in preparing the first designs for a graphite-moderated gas-cooled nuclear power plant in 1947 and 1948. Although the scheme which was evolved at that time was clumsy it proved the conception to be practical and formed the foundation for the design study work which was done under Goodlet at Harwell in 1952 and 1953 and which in its turn provided the framework for the Calder Hall design. Gibb's engineers formed part of the Harwell team and later of the Calder Hall team at Risley and he took a close personal interest in the work. The turbo-alternators and the gas circulating blowers at Calder Hall were supplied by Messrs C. A. Parsons.

When it was decided that industrial nuclear power plants were practical and that the responsibility for their design and construction should be given to engineering firms, Messrs C. A. Parsons were naturally one of the four great electrical firms who were brought in and asked to form consortia. Gibb quickly brought together eight companies who were already skilled in particular aspects of nuclear engineering and formed them into the Nuclear Power Plant Company Limited with its design headquarters at Knutsford. The Company received one of the first two orders for industrial nuclear power plants and the station at Bradwell in Essex is their responsibility.

Supplies of the special graphite which is required for the cores of reactors of the Calder Hall type had always presented difficulty to the Atomic Energy Authority and they found themselves short of the tonnage required for the expanded nuclear programme on which the Government decided in 1957. With typical energy Gibb negotiated an agreement with the Atomic Energy Authority under which he formed a joint company with the Great Lakes

Carbon Corporation of America and built a factory to produce 10 000 tons a year of nuclear grade graphite and 5000 tons a year of commercial grade graphite. The agreement was completed in February 1957, the factory was built, and the first furnace load of finished graphite was dispatched in June 1958. The speed was achieved only as a result of Gibb's personal drive and direction in the whole period of negotiation and construction.

Claude Gibb had an honest and healthy liking for publicity; it arose not merely from personal taste for it but also from a proper evaluation of its business value and from an honest feeling that it was something that he owed to the public. He was able to speak easily and naturally on a wide range of subjects because he had a clear brain, a good memory and wide experience. He never hesitated to state the opinions which he held, quite regardless of whether they would be unpalatable to his hearers or embarrassing to other people. As a lecturer he had a high reputation and regarded lecturing as a professional duty.

In 1949 his firm had supplied two 100 MW turbo-alternators which were installed in the Toronto Generating Station of the Hydro Electric Power Commission of Ontario; the first went on to load in October 1951 and the second in 1952, both failed within a few days of each other in April 1954. The effect was disastrous, in both cases serious fires followed the mechanical failure and in both cases the generator stator and its rotor were completely wrecked. When the first failure occurred two of Gibb's most senior men flew immediately to Canada, made a first diagnosis of the cause of failure and acting in collaboration with them he put replacements in hand so quickly as to surprise the whole power plant world. As soon as the investigations were complete Gibb read a paper on the cause of the failure to the Institution of Mechanical Engineers in which he dealt with the incident so explicitly and so frankly as to turn failure into triumph. His papers to the Institution won him the Hawksley Medal on two occasions. The Parsons Memorial Medal and the James Watt International Medal for distinction as a mechanical engineer were also awarded to him by the Institution. He was given Honorary Degrees by the Universities of London and Durham.

Gibb was elected a Fellow of the Royal Society in 1946, was a Member of the Council in 1955 and Vice President in 1956. He was also a Member of the Council of the Institution of Mechanical Engineers from 1936 until 1954 and Vice-President from 1945 to 1951. He retired from the Council only because of the pressure of other outside engagements. These engagements were heavy; he actively supported the British Association and was President of the Engineering Section in 1951, he was a Member of the Ridley Committee which investigated the efficient use of coal, gas and electricity, he was a Member of the President of the Board of Trade's Informal Advisory Group on Exports, Chairman of the Council of the International Electrical Association, Chairman of the Athlone Fellowship Managing Committee, Chairman of the Committee on the Organization and Control of Government Research Expenditure and a Member of the Council of King's College, Newcastle upon

Tyne. He was created Knight Bachelor in 1945 and was made a Knight Commander of the British Empire in 1956.

In 1948, when he was 49 years old, Claude Gibb had a severe coronary thrombosis and for a time his condition was critical. He made a complete recovery but he had to live on a diet so restricted that he used to say that he was always hungry; although he was supposed to avoid undue physical strain he worked and travelled as restlessly as ever. Five years later he had a second coronary thrombosis and a third followed while he was still in hospital; again he made a complete recovery and again returned to all his old activities.

At the end of 1958 his firm submitted a tender for a 500 MW turbo-alternator to the Tennessee Valley Authority in competition with American firms. Claude Gibb flew to America with two engineers in January 1959 to discuss the tenders. When the technical discussions were completed his two assistants flew to Canada where he was to join them for further business negotiations; he stayed at Knoxville for a final meeting with the Chairman of the Tennessee Valley Authority. He left by air on 15 January and while waiting at Newark Airport for the plane which should have taken him to Canada he collapsed and died. His firm received the order for the 500 MW set from the Tennessee Valley Authority the following week.

All his friends knew that one day they were bound to read that he had died in this way. Yet it was surprising how little the sense of inevitability softened the blow. He was a man who, though he could at times be quite infuriating to his friends, won not merely respect but also deep affection from all the people who worked for him or with him.

As an organizer he was clear, firm and methodical, but like many men who are full of energy and are supremely confident he found it difficult, until his last years, to delegate responsibility to others. As a business man he was astute and far sighted. He was an outstanding engineer of a type which is unfortunately rare; an engineer who was at home in the design office, who was proud to use workshop tools and machines as a craftsman, and yet who had a thorough grasp of the scientific theory on which the art of engineering rests.

C. Hinton

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J Jackson

JOHN JACKSON

1887-1958

John Jackson was born on 11 February 1887, the fifth of eight children born to Matthew Jackson and Jeannie (*née* Millar). His father was a skilled mechanic, a craftsman who could have risen if he had not preferred practical work to directing and supervising other workmen. His parents were keen on education and encouraged their children to take advantage of such opportunities for higher education as were available. Jackson's elder brother, Robert, after obtaining the M.A. degree at the University of Glasgow, became classical master at Paisley Grammar School. Jackson's early education was at the North Public School, Paisley, 1892-1899, and then at the Camphill Public School, 1899-1900. At the age of 13 he entered the Paisley Grammar School, where he took a curriculum including French and German as well as science subjects, but not including Latin or Greek. There had evidently at that time been no intention of proceeding to the University, as Latin (or Greek) was then compulsory for the entrance examination.

When Jackson left school in 1903 at the age of 16, he had done well in the science subjects and in particular in chemistry. He decided to try for the entrance examination at the University of Glasgow despite his ignorance of the classics. He had done a little Latin before entering the Paisley Grammar School, and during the summer holidays of 1903 he studied hard to improve his knowledge of the subject. He managed to pass the entrance examination for the University sufficiently well to be awarded a £25 bursary. At that time the Carnegie Trust for Scottish Universities provided funds to pay the class fees which made it financially possible for Jackson to enter the University.

He had considered chemistry to be his best subject and had intended to continue its study as his principal subject at the University.

His course of studies included mathematics and natural philosophy and he soon became top of his year in mathematics. In 1907 he graduated M.A. with first class honours in mathematics and natural philosophy and was then awarded a Ewing Fellowship of £100 a year for further study, together with some slight tutorial duties. In 1908 he took the degree of B.Sc. with special distinction in mathematics, natural philosophy, astronomy and chemistry. During the course of his University career at Glasgow, he was a Medallist in botany, in natural philosophy and in mathematics.

It was well known amongst the students taking honours in mathematics that they would benefit by attending the astronomy classes given by Ludwig Becker, the Regius Professor of Astronomy. Jackson attended these classes and obtained a thorough grounding in both spherical and dynamical astronomy to the extent required for a complete understanding and even compilation of the *Nautical Almanac*. He learnt the adjustment and uses of the principal astronomical instruments, the determination of the corrections required by the observations, and how concrete physical problems could be solved to any required degree of accuracy by arithmetical methods when mathematical analysis was unable to yield a solution. Each student attending the classes was required to compute the ephemeris of a minor planet from the elements of its orbit.

Before attending Professor Becker's classes Jackson had no knowledge of or interest in astronomy, but he was profoundly interested by the enthusiastic systematic teaching of Becker, which made him wish to find a career in astronomy. In later life he often referred to the influence that Becker had exerted on him and to the extent to which he was indebted to him.

As there seemed no prospect of obtaining an appointment in astronomy from Glasgow, Jackson decided to go to Cambridge if possible. He entered for the entrance scholarship examination at Trinity College, Cambridge, and was awarded a major scholarship. He resigned his Ewing Fellowship at Glasgow and entered Trinity College as an ordinary undergraduate in 1909. He already knew as much spherical and dynamical astronomy as was then taught at Cambridge, but he was introduced there to solar physics and astrophysics and he read extensively descriptive astronomy, of which he had previously known practically nothing. His knowledge of pure and applied mathematics was extended, especially through the lectures of R. A. Herman and G. H. Hardy. He afterwards said that it would have been much better if he had gone to Cambridge two years earlier.

In 1910 he obtained a first class in Part I of the Mathematical Tripos and was awarded the Sheepshanks Exhibition. He took Part II of the Tripos in 1912, obtaining a first class, with the b* for distinction in advanced subjects, and was awarded the Tyson Gold Medal in Astronomy. In 1914 he was awarded a Smith's Prize. The previous year he had been appointed a Mackinnon Research Student of the Royal Society. The eighth satellite of Jupiter, which has a retrograde orbit, had been discovered by Melotte at the Royal Observatory, Greenwich, in 1908. There was no reliable orbit of this satellite available from which its position at any future time could be predicted, and because of its faintness its rediscovery was uncertain. Jackson took as the subject of his studentship the study of the orbit of the satellite. His first published papers were concerned with a discussion of the discordances between the observed and predicted positions of the satellite, and with the general problems of retrograde satellite orbits.

Jackson did a great deal of computational work on the orbit of J VIII, calculating the elements of the orbit from the rectangular co-ordinates at rather close intervals of time with the intention of expressing the elements in series of periodic terms, the arguments of which were indicated by Delaunay's Lunar Theory.

The perturbations proved to be too large, however, to lead to useful results by this method. He was therefore led to the study of Hill's papers, as Hill's development of lunar theory gave series that were much more convergent. At about this time, in conjunction with F. J. M. Stratton, he edited Vol. V. of the *Scientific Papers of Sir George Darwin*, published by the Cambridge University Press in 1916.

In 1914 a vacancy as Chief Assistant occurred at the Royal Observatory, Greenwich, through the resignation of S. Chapman, and Jackson was selected by the Astronomer Royal, Sir Frank Dyson, to fill the post. He thereupon resigned the Mackinnon Studentship and entered upon the career in astronomy on which he had set his heart. At that time the staff of the Observatory was much depleted, the younger members of the staff and all the unestablished computers, who then took a large share in the routine observing programmes, having left for war service. Jackson consequently undertook a large amount of observing, particularly with the Airy Transit Circle; throughout the rest of his life he continued to take pleasure in astronomical observing. On one occasion, during a Zeppelin raid, while Jackson was observing, a bomb fell in the Observatory grounds. The Astronomer Royal urged Jackson to take shelter, but he continued to observe quite unperturbed, with the characteristic remark, 'The Observatory was founded to obsairve the Moon and I'm going to obsairve the Moon'.

The Observatory was responsible for the time service of the country and for the care and testing of chronometers and watches for the Navy. As a war effort, the cleaning and adjusting of large numbers of binoculars for the Optical Inspection Department of the Royal Arsenal, Woolwich, was undertaken, and a lookout was also kept for Zeppelins. In all this work Jackson took a full share until near the end of 1917 when he was granted a commission in the Royal Engineers. After taking short courses at Chatham and Southampton, he went to France to work on sound ranging; but the critical position of the Allied armies in the spring of 1918 resulted in his being sent to the Fourth Army as a trigonometrical survey officer. This was work for which he, with his experience in observing and his basic knowledge of spherical astronomy, was well qualified. Until the termination of hostilities he was engaged in this work in connexion with the heavy artillery. Known survey points were used when possible, but when they were not available, he resorted to observations of the sun in order to determine azimuth.

Jackson returned from France in February 1919 to resume his duties at the Observatory. The first extensive task he undertook, at the suggestion of the Astronomer Royal, was the preparation for publication of all the observations of double stars that had been made at Greenwich with the 28-inch refractor during the years 1893 to 1919. A number of observers had shared in the observations, and the measured distances and position angles had been published year by year in the annual volumes of *Greenwich Observations*, but they had never been collated and discussed. Jackson prepared the observations for publication, used the Greenwich observations in conjunction with observations made elsewhere to derive the orbits of 24 binary systems, and proceeded to derive 'hypothetical' parallaxes for many of the stars. This work, which was published in a Greenwich volume, for which Jackson wrote the introduction and notes, led to his life-long interest in double stars.

If the orbit of a binary system is known, the parallax, p, of the system can be deduced from the relationship

$$(m_1+m_2)\ p^3P^2 = a^3$$

where P denotes the period of the system in years, a is the semi-axis major of the system in seconds of arc, p is expressed in seconds of arc, and m_1, m_2 are the masses of the components of the system in terms of the mass of the sun as unit. There were 130 binary systems for which sufficiently reliable orbits were known. The masses of the stars are not in general known but, since the parallax enters into the above formula with the third power, the derived parallax is not greatly affected by any error in the adopted masses. Stellar masses do not have a large range and it is sufficiently accurate to assume that the combined mass of a binary system is twice the solar mass. The parallax deduced in this way is termed the 'hypothetical' parallax.

Many other binary systems show relative motion in distance and position angle, but have not completed an orbital revolution since they were first observed. Jackson derived a formula from which the hypothetical parallax could be inferred from the observed rates of change of position angle and distance with time. In collaboration with H. H. Furner, he used the Greenwich observations, in conjunction with observations made elsewhere, for the determination of hypothetical parallaxes of all known binary systems showing distinct orbital motion. The parallaxes of 556 stars were derived in all, providing an important addition to the knowledge of stellar distances available at that time. He used the hypothetical parallaxes to compute the cross linear velocities of the stars from their proper motions, and then to derive not only the direction but also the speed of the solar motion. The agreement of the solar velocity inferred in this way with that derived from the observed line-of-sight motions of the stars provided a justification of the assumption about the masses of the binary systems that had been made in computing the parallaxes.

In the course of this work on double stars, Jackson contributed 9 papers to the *Monthly Notices of the Royal Astronomical Society*. While examining the original work of Wilhelm Struve he discovered how much Struve had been ahead of his time in predicting which stars were nearest, before the distances of any stars had been directly determined, and that he had even calculated hypothetical parallaxes of some binary stars. The important discussion of these matters by Struve had long been overlooked and forgotten.

Amongst the miscellaneous papers published by Jackson at about this time was a short paper on the figure and rotation period of Neptune. Photometric observations of the planet had shown that its brightness was variable in a period of about 7·8 hours and it had been assumed that this was also the period of rotation of the planet. From the rate of motion of the pole of the satellite's orbit about the pole of the planet's equator, he showed that on any reasonable assumption as to the figure of the planet, the period of rotation must be longer. He inferred a value of 19 hours with a possible error of about 20 per cent and concluded that the period must be two or three times that derived from the photometric observations, which could be attributed to brighter or darker areas on the planet at intervals of approximately 180° or 120° in longitude. The rotation period now accepted is 15·8 hours, twice that concluded from the photometric observations.

When Jackson went to the Royal Observatory, the determinations of time were made with the Airy transit circle and the standard clock was a Dent regulator made in 1874. The clock could suddenly alter its daily rate by as much as 0·05 second, though the erratic character of the clock corrections derived from the transit circle observations made an accurate control of the clock's performance impossible. The inception of wireless time signals from Paris, Norddeich and Annapolis made possible the comparison of the time determined at Greenwich with the time determined elsewhere and showed clearly the need for the Royal Observatory to be provided with a more accurate standard clock. The Astronomer Royal made several attempts to obtain a better clock and finally, in November 1924, a Shortt Free Pendulum clock was installed. The prototype clock of this type, Shortt O, had been installed at the Royal Observatory, Edinburgh; its performance had been studied by R. A. Sampson, the Astronomer Royal for Scotland, and had been given a favourable report. A small transit instrument, reversed during the transit of each star so as to eliminate collimation error, was substituted for the Airy transit circle for the determination of time and proved to be superior for this purpose.

It quickly became apparent that the Shortt clock gave a much better performance than any previous clock, and it was brought into use as the standard clock of the Observatory at the beginning of 1925. Jackson, in collaboration with W. Bowyer, who was in charge of the time service, studied its performance. Daily readings were taken of the arc of vibration of the master pendulum, of the pressure in the sealed clock case (which had a slight leak), and of the temperature in the clock cellar. The effects of changes in temperature and pressure on the rate of the clock were determined. It was found that the optimum pressure at which to maintain the clock case was 18 mm of mercury as, at that pressure, a small change in pressure had no effect on the rate of the clock; the change of rate due to change in the arc of vibration of the pendulum and the change consequent upon the change of resistance of the air in the clock case compensated each other. After allowing for the effects of changes of pressure and temperature on the performance of the clock, it was found that most of the remaining irregularities could be explained by the fact that sidereal time, which is affected by the nutation of the earth's axis, is not uniform. This was the first time that a clock had been found to be more uniform than sidereal time and it led to the introduction of *mean sidereal time*, freed from the effect of nutation, in time keeping. Jackson also investigated the possibility that Shortt clocks might be used for the control of irregularities in the rate of rotation of the earth. Comparison of the observed positions of the Moon with positions derived from gravitational theories of its motion had proved that the earth's rotation is not uniform. He concluded that, if the fluctuations in the observed rate of the clock, after correcting for nutation and for changes in temperature, pressure and arc of vibration, were attributable to changes in the rate of rotation of the earth, the observations of the Moon's position were not sufficiently accurate for any conclusions to be drawn.

The next extensive piece of work undertaken by Jackson was in collaboration with H. Knox-Shaw, the Radcliffe Observer and Director of the Radcliffe Observatory, Oxford. The Trustees of the will of Dr John Radcliffe, who died on 1 November 1774, had built, in the latter part of the 18th century, an observatory for Thomas Hornsby, the Savilian Professor of Astronomy, who was appointed the first Radcliffe Observer, a post held in conjunction with the Savilian Professorship. During the years 1774 to 1798, Hornsby made observations at the Radcliffe Observatory of the positions of the Sun, Moon, planets and stars. He had observed so industriously throughout those years that no time was left available for the reductions of the observations. They had remained unreduced and no use had been made of them. The observations were known to be of high quality and because of their early date promised to be of special value for checking the accuracy of the fundamental star system in use in the 20th century. Knox-Shaw and Jackson decided to collaborate in the reduction of Hornsby's observations. There were about 43000 observations of stars, 5200 of the Sun, 4600 of planets and 1200 of the Moon.

Knox-Shaw, assisted by W. H. Robinson, used the observations to determine the errors of the instrument, applied the necessary corrections, and deduced the apparent places of the objects observed. Jackson undertook the task of computing with modern accuracy the apparent places of the Sun and stars from the best available tables, as the *Nautical Almanacs* for the epoch of the observations did not have the required accuracy. The observations of the Sun were required for checking the position of the equinox. The positions of the Sun were computed from Newcomb's *Tables of the Sun* for every Greenwich mean noon and every Oxford apparent noon for the years 1774-1798. Copies of the results—required for computing the positions of planets and comets as seen from the earth—were deposited at Greenwich, the Royal Astronomical Society, Washington and Berlin. The apparent places of the stars were based on Boss's *Preliminary General Catalogue*. The results proved that the motion of the equinox and the proper-motions adopted in that catalogue were substantially correct. The positions of the planets from Venus to Saturn were calculated by A. C. D. Crommelin and B. F. Bawtree. Jackson undertook the comparison of theory and observation for Mercury and verified the motions of the perihelion of that planet adopted by Newcomb, and now known as the most accurate confirmation of Einstein's theory of generalized relativity. These investigations were assisted during six years by grants from the Royal Society.

In 1930 the planet Pluto was discovered at the Lowell Observatory, Flagstaff, Arizona. Jackson was specially interested in the predictions of Neptune and Pluto from the analysis of the perturbations they produced. He emphasized that for the discovery of the perturbing planet it is not necessary to predict all the elements of its orbit, but only its direction and its approximate rate of change. He always considered that the discovery of Pluto was purely fortuitous as, with the other elements of its orbit known (from subsequent observation), its mass cannot be determined with any accuracy. In a paper published soon after the discovery of Pluto he showed that the differences between the tabulated and observed positions of Neptune could be explained by a slight change in the orbital elements of the planet.

Concerning the optical discovery of Neptune, Jackson wrote a defence of Challis, who has been much blamed for his failure to discover it before the Berlin observers; in his search for the planet he had made observations of it, but had not reduced the observations, which would have revealed its motion relative to the stars and would have resulted in its discovery. Jackson pointed out that everybody except Leverrier considered that the discovery of the planet would be very difficult. It seemed to be a case of hunting for a needle in a haystack. Challis set out on a laborious task—to find the planet if it were anywhere near the predicted place. He had only a few months to complete an extensive survey before the planet would be lost in the sun's rays, after which he could examine his observations at leisure. It was his misfortune that the Berlin observers, with the aid of a chart of the region of the sky that included the predicted place of the planet—which was not available to Challis—at once discovered the planet. If they had failed to find it, Challis would have done so and would have been hailed as the hero who undertook his heavy task in an eminently scientific manner. For him there was no race to be first and for him a month or two did not matter.

In 1933 there occurred a vacancy in the post of H.M. Astronomer at the Cape, consequent upon the appointment of H. Spencer Jones to succeed Sir Frank Dyson, who was retiring from the office of Astronomer Royal. Jackson was appointed to the vacancy. The main programmes of observation at the Cape Observatory had been laid down by his predecessor and were carried on by Jackson throughout his tenure of the post of H.M. Astronomer. The Cape Astrographic Zones, −40° to −52° declination, had been rephotographed and the plates measured against the original plates for the determination of the proper-motions of the stars. On each plate suitable comparison stars of about 11·9 photographic magnitude had been selected; all stars down to a magnitude limit of 9·5 photographic were measured; in addition, all other stars on the plates were examined, and those showing a relative motion exceeding about 5 seconds of arc per century were measured. Upon Jackson fell the duty of discussing the derived proper motions and preparing the results for publication. The spectral types of about half the stars were given in the *Henry Draper Catalogue*; the types of the remaining stars, except for some of the faintest, were provided by the Harvard College Observatory. For this purpose, photographic prints, on which each star was identified, were supplied by the Cape Observatory. The proper-motions were published, together with the spectral types, in two Cape volumes. The first related to the 20843 stars whose positions had been published in the Cape Zone Catalogue, —40° to —52° dec.; the second related to 20554 faint stars. The introductions to these volumes were written by Jackson, who also discussed the distribution of the stars with respect to photographic magnitude, spectral type and proper-motion in a paper in *Mon. Not. R. Astr. Soc.* **98,** 491 (1938).

Before Jackson was appointed to the Cape, an extensive programme of determination of the positions of stars south of —30° dec., by photography with a wide-angle lens, had been planned. This programme was designed to complement the programme of the Yale University Observatory, which at New Haven and Johannesburg had undertaken the preparation of zone catalogues from the North Pole to —30° dec. by photography. The lens had been constructed by Messrs Taylor, Taylor & Hobson, the camera had been designed and made in the Observatory workshop, and the first zone, —30° to —35° dec., had been photographed but had not been measured. It was intended to determine the photographic magnitude of each star and, with this in view, a Schilt photometer had been obtained and installed, though this portion of the programme had not been commenced. The work of observation was continued throughout the years that Jackson was at the Cape. At the time he retired, all the zones to —80° dec. (except for —40° to —52°, which were covered by the Cape Astrographic Zones) had been photographed; comparison stars had been selected and their positions determined with the reversible transit circle; the measurements had been completed to about —68° dec.; the zones —30° to —35° and —35° to —40° had been prepared for publication, except for exact magnitudes, which were being rapidly completed. The magnitude determination programme was entrusted by Jackson to his Chief Assistant, R. H. Stoy, who undertook a series of investigations by means of which the photometry of southern stars, which had been in an unsatisfactory and chaotic state, was established on accurate and firm foundations.

The programme that occupied most of Jackson's time at the Cape was the determination of stellar parallaxes with the 24-inch Victoria telescope. This programme had been commenced in 1926. At that time the parallaxes of a large number of stars in the northern sky had been determined by photographic methods but there were few reliable determinations of parallaxes of southern stars, principally those that had been made by visual observations with the 7-inch heliometer at the Cape. Spencer Jones, in collaboration with F. Schlesinger, Director of the Yale University Observatory, decided to embark on programmes of parallax determinations of southern stars to repair the disparity between the northern and southern sky. The Yale telescope was installed by Schlesinger in the grounds of the Witwatersrand University, Johannesburg. It was agreed that the Yale telescope should observe all the brighter southern stars while the Cape Observatory would observe primarily stars of large proper motion, many of which are faint. Bright stars with large proper motion were common to the two programmes, thus permitting the examination of possible systematic differences between the two series of determinations. By the time Jackson reached the Cape, the parallaxes of more than 400 stars had been determined.

He took over the selection of stars for observation, and obtained the first few plates of each star so as to determine the exposure time required to photograph suitable comparison stars and the occulting-shutter aperture required for the reduction of the brightness of the parallax stars to equality with the brightness of the comparison stars. As time went on, particularly during World War II when the staff of the Observatory was much reduced, he took over more and more of the work of the department in observing and calculating. He obtained as many as a thousand plates in a year. When he retired, the detailed results for 1600 stars were ready for publication, and have since been published in three volumes of the *Cape Annals*. The Cape and Yale programmes of stellar parallax determination have together removed the disparity that had existed between the knowledge of parallaxes of southern and northern stars; the knowledge of stellar parallaxes is in fact now somewhat better for the southern sky than for the northern.

During the War, about half of the staff of the Observatory was released for war service. With the remaining staff, Jackson concentrated on observations which, if not made at the time, could not be replaced by others at a later date. The reductions of the observations were necessarily deferred to a large extent. In this way, and inspired by Jackson's enthusiasm and participation, the Observatory was able during these years to make contributions to astronomy that were comparable with those that would have been made in normal times by the full staff.

Jackson took part in four expeditions to observe a total eclipse of the Sun. On 29 June 1927 occurred the first total eclipse of the Sun since those of 2 May 1715 and 22 May 1724 in which the path of totality crossed England. The Greenwich Observatory sent an expedition to Giggleswick, Yorkshire, in which Jackson participated, to photograph the corona. The eclipse took place shortly after sunrise so that the sun was very low, and was of short duration. The weather conditions along the path of totality in England were generally unfavourable, but Giggleswick proved to be one of the few places in which the total phase of the eclipse was observable. Jackson was the leader of two expeditions from the Observatory to observe subsequent total solar eclipses, to Alor Star, Malaya, for the eclipse of 9 May 1929, when observations were prevented by thick cloud, and to Parent, Canada, for the eclipse of 31 August 1932, when observations of the flash and coronal spectra using a slit spectrograph were planned but were prevented by cloud. The Greenwich Observatory had planned to send an expedition to South Africa to observe the total eclipse of 1 October 1940. Jackson made investigations of the conditions for the observations of this eclipse and selected Calvinia, Cape Province. The outbreak of World War II made it impossible to send observers from Greenwich, but the instrumental equipment, which was designed for the determination of the displacement of stars in the vicinity of the Sun predicted by Einstein, was sent to the Cape. Jackson took charge of an expedition from the Cape Observatory to Calvinia. The sky was clear throughout the eclipse but the definition proved unfortunately to be very bad so that the images of the stars were much too diffuse for accurate measurements to be possible. After his retirement from the Cape he went to Strömstad, Sweden, to observe the total eclipse of 30 June 1954 as a spectator and was able to see the total phase in a veiled sky.

Jackson was Secretary of the Royal Astronomical Society, 1923-29, and President 1953-55. He was awarded the Gold Medal of the Society in 1952 for his work on stellar parallaxes and his contributions to the general problems of star positions and proper motions.

He was Secretary of Section A of the British Association, 1919-24. He was President of Commission 8 (Meridian Astronomy) of the International Astronomical Union, 1932-38, and of Commission 24 (Stellar Parallaxes and Proper Motions), 1938-52, and was responsible for the preparation of the reports of these Commissions to the General Assemblies of the I.A.U. held during the respective periods. He was elected a Fellow of the Royal Society of South Africa in 1934, served on its Council for many years and was elected President in 1949. In 1958 he was awarded the Gill Medal of the Astronomical Society of South Africa.

On his retirement from the Cape, the honour of Companion of the Order of the British Empire was conferred upon him in the 1950 Birthday Honours. He returned to England and made his home in Ewell, Surrey. He died on 9 December 1958 after a brief illness. In 1920 he married Mary Beatrice Marshall, who survives him: they had one son who died shortly after birth.

He was elected a Fellow of the Royal Society in 1938.

H. Spencer Jones

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K S Lashley

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KARL SPENCER LASHLEY

1890-1958

In July 1949 the Society of Experimental Biology and the Institute of Animal Behaviour together organized a Symposium for the discussion of a wide range of problems, neurological, physiological and psychological, in which the members of both groups were interested. The Symposium was held in the Zoological Laboratory of the University of Cambridge. K. S. Lashley came to England for this meeting, and no member of the large audience who heard him describe how he had set out 'In search of the engram', and what its upshot had been, is likely ever to forget the tremendous impression that he made.

After the public discussions one morning, I walked with him back from the Zoological Department to Corpus Christi College, where he was staying. The sun shone with unclouded brilliance, and when we reached the College Lashley, who seemed, perhaps, a little tired, sat himself down on the stone steps leading to the main gate, one long leg stretched out towards the pavement, with not a single care for the curious, and slightly shocked glances of some of the passers-by. We were talking, not about any intricacies of animal behaviour, but about sailing and the sea, which he loved. He told me something about his own boats, and journeys he had made in them; but more about longer and unconventional voyages in small tramp steamers which took a long time, wherever they were going, and called at ports little known to the big luxury vessels for which he had no use at all.

Here was Lashley, sitting where hardly anybody ever did sit, talking quietly and naturally about things that I suppose very few, if any, of the others who came to that conference could, or would, talk about. It was an occasion which I am glad that I can remember. For, as I see them, all Lashley's scientific explorations, most of them planned and carried out with infinite care, and all the exercise of his superb intelligence, were a prolonged and serious adventure. In his experimental and theoretical work, and in his life outside the laboratory as well, he would go where he wanted, he would say what he believed, not because he was insensitive to influences from other people, but because his only ultimate compulsion came from inside himself.

Lashley was born in Davis, West Virginia, on 7 June 1890. In some notes which he made about his ancestry he wrote 'Lashley is an old English name and the family is certainly of English descent. To judge from their distribution Lashleys were probably early immigrants to the Carolinas and spread from there, but I have no definite information beyond the third generation. My great-grandfather, Henry Lashley, was in the tanning and brick-making business, with interests in farming'. Karl's grandfather developed further various trading activities, and in due course established his father, Charles Gilpen Lashley (1860-1938) in a general store at Davis in West Virginia. Charles Lashley had 'hoped to read medicine with a local doctor', but the opportunity did not come. 'As I knew him', wrote Karl, 'his only interests were in his business, family, and local politics'.

Lashley's mother's family also came from England, and in America engaged mainly in fairly large-scale farming. His grandmother on this side came from the family of the philosopher Jonathan Edwards, and 'it is clear that intellectual curiosity and drive descended through that maternal line. My mother, Maggie Blanche Spencer (1860-1933) started to teach in Maryland country schools at the age of sixteen. She obtained a normal school education and a reputation for handling disorderly schools. She had a great reverence for learning, was an avid reader, and collected a library of more than 2000 volumes. Shortly after her marriage she opened a photographic studio; later she turned to painting, which was the chief interest of her later years'.

Karl was an only child. His early boyhood experiences were varied. When he was still very young, the family moved to California for a short time and then, in 1898, joined in the gold rush to the Klondike. This journey into Alaska was apparently mainly fun and adventure, for a year later they were back again and re-established in Davis. But the trip made a tremendous and lasting impression on Lashley so that twice in his later life he repeated the same journey.

One of the consequences of these early travels was that Lashley had comparatively little routine school life. This was of small moment, for obviously he learned all that was needed, both in the way of regulation school accomplishments and in effective interests, from his own mother. However, he did have four or five years at the Davis High School. and already, when he left, some of the abiding interests of his life were established. He had become a keen student of the natural behaviour of animals; he was attracted by gadgets of all kinds and was clever with his hands; he liked to get hold of a couple of logs, fasten them together, and paddle himself about on the river.

In 1905 he became a full student at the University of West Virginia. He had been somewhat undecided what subjects to specialize in, and it seemed like one of those odd chances that often happen early in a brilliant career, that took him into a course of zoology taught by the neurologist, John Black Johnston. Later he was to write: 'Within a few weeks in his class I knew that I had found my life work.' In 1906 he became a research assistant to Albert M. Reese who had succeeded Johnston. He found one day '. . . a beautiful Golgi series of the frog brain. I took these to Reese and proposed that I draw all of the connexions between the cells. Then we would know how the frog worked. It was a shock to learn that the Golgi method does not stain all cells, but I think almost ever since I have been trying to trace those connexions.'

In 1910 Lashley obtained his A.B. degree, and won a teaching fellowship in bacteriology at the University of Pittsburgh, where he became Master of Science in 1911. It was during this stage that he came directly under the spell of experimental psychology. In the first place this was due to his association with Karl Dallenbach, who was himself to become a leading psychologist in America, and the tie was further strengthened when, in the following year, he enrolled for a Ph.D. at Johns Hopkins. At first he worked there chiefly with H. S. Jennings on inheritance in paramecia, but the brilliance of his promise and his rather unconventional temperament, attracted both J. B. Watson, the behaviourist, and Adolph Meyer, the outstanding psychiatrist. In 1912 he published his first paper in the *Journal of Animal Behaviour*. In a way this was prognostic. It was entitled 'Visual discrimination of size and form in the albino rat'. In the 1930's and early 1940's, when he was at the very height of his powers, he was to come back with a long series of penetrating experimental studies of the animal visual mechanisms and their functions.

Somewhere around 1915 Lashley definitely decided that he would make his career in psychology, and from this decision he never swerved. The psychology that attracted him, however, was not the rather prim and respectable laboratory exercises of the earlier Germans, which were being imitated in those American Universities that allowed psychology a place in the structure of their studies; and it certainly was not introspectionism run to extremes. He was now working fairly closely with J. B. Watson, and Watson was as much of an adventurer in the fields of animal and human behaviour as he was himself, though nowhere near as cautious. Lashley had had several years of training and research in zoology, together with a fair amount of anatomy and physiology, and some neurology. All of these, in their relation to the behaviour of animals, seemed to be impelling him towards an experimental examination of the directions, character and final upshot of conduction in the central nervous system. There have been plenty of psychologists who, having made a careful firsthand study of behaviour, have been willing to speculate about its underlying physiological and neurological conditions, though of these they usually could claim no more than an inadequate and second-hand knowledge. It has perhaps been still commoner for people who have come from one or more of the other biological sciences, when they get to the level of behaviour itself, to suppose that no specially controlled observation and experiments are needed. In this field, they seem to assume, nothing is required beyond the exercise of common sense or the acceptance of established views. It was a part of Lashley's great strength and originality that he was, all his scientific life, prepared and able to take one side as seriously and as experimentally as the other.

For a few years still Lashley could, and did, allow free play to a lively curiosity that took him in many different directions. Students and colleagues who knew him well then have recorded that, though what he would say or do next was highly unpredictable, there was all the time a kind of 'contagion of excitement' about him. With Watson he made a study of homing. He published observations on the nesting activities of the noddy and sooty terns, and on colour vision in birds. He made an excursion into human learning with an account of the acquisition of skill in archery. Washington, D.C., being easily accessible to him, he made contact with Dr S. I. Franz at St Elizabeth's hospital and two joint papers with Franz, on the effects of cerebral destruction on habit formation and retention in rats began to indicate where his future work was to lie.

In 1917 Lashley went as Instructor in Psychology to the University of Minnesota. But the conditions were not very promising and a year later he was back again in Washington, and off on another new track. He accepted a post with the U.S. Interdepartmental Hygiene Board to work with Watson on popular views about sex. Both of them seem to have got some fun out of this, but Lashley took it seriously too, and later was for a number of years a member of the N.R.C. Committee for Research on Problems of Sex.

The years of concentrated and closely directed effort were now approaching. In 1920 he was back at Minnesota as assistant professor in psychology and free from routine teaching. Four years later he became full professor. In 1926 he moved to Chicago. There he was given great freedom to pursue research, and eventually he became Professor of Psychology at the University in 1929. In 1935 he was appointed Professor of Psychology at Harvard, and then, in 1937, Research Professor of Neuropsychology, with only that amount and kind of teaching to do that he himself desired. This allowed him, in 1942, to accept also the Directorship of the already famous Yerkes Laboratories of Primate Biology. He now made his home in Florida and continued there until and after his retirement to emeritus status in 1955.

The years 1920-29 were crowded with original experimental exploration. This was by no means all along one single stream, but as he said himself he was, in the main, following his early determination to find out all he could about links and lines of connexion in the central nervous system, and their function in regard to sensory discrimination, and the formation and retention of habits in animals.

In 1929 he was elected President of the American Psychological Association and he delivered, in his Presidential address, a whole-hearted attack on those behaviour theories which derive everything from reflex actions and combinations of reflex actions. This year, also, he published *Brain mechanisms and intelligence: a quantitative study of injuries to the brain.** It was a beautifully economical summing up of his experimental work of the preceding nine years. It quickly achieved wide recognition, and whatever may eventually prove to be the finally correct story in detail of the nervous mechanisms and their functions involved in intelligent behaviour, it is certain that Lashley's work and his exposition in this monograph will remain of fundamental importance.

In this book Lashley stated his two principles of equipotentiality ('. . . The apparent capacity of any intact part of a functional area to carry out, with or without reduction in efficiency, the functions which are lost by destruction of the whole. This capacity varies from one area to another and with the character of the functions involved. It probably holds only for the association areas and for functions more complex than simple sensitivity or motor-coordination', p. 25) and mass functions* ('. . . whereby the efficiency of performance of an entire complex function may be reduced in proportion to the extent of brain injury within an area whose parts are not more specialized for one component of the function than for another' *loc. cit.*) In view of the incautious use which commentators have sometimes made of these two principles, it is necessary to point out that they are inductive generalizations from a mass of data in which the determining neurological and physiological conditions, and their detailed issue in behaviour, were observed with equal care. They were never put forward as themselves explaining anything, but only as summarizing in a general way a great amount of penetrating experimental observation. His third principle, of disturbances of equilibrium within functional systems following unilateral brain lesions—to which, for some reason, less attention has been paid—is of the same order. All three principles are not explanations, but are summary statements of that which has to be explained. Lashley himself made this perfectly clear.

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* University of Chicago Press.

From now until 1955, and the onset of an obscure blood disorder, Lashley continued to live an extremely active life as an experimenter. He returned to his early interests in the mechanisms of vision, and linked closely with this many further studies of cerebral function in learning. Following his appointment at Harvard, and particularly as Director of the Yerkes Laboratories, naturally more and more of his own and his students' work concerned the behaviour of primates. Always it was intelligent aspects in behaviour that compelled his attention. He remained true to his early demonstrations that intelligence operates at levels of performance—e.g. simple maze-running—which others often have tried, and still continue to try, to put into terms of an automatic conditioning process, and at the same time he was turning more frequently to somewhat general problems, and to investigations in the field of the 'higher mental processes'. It was in the line of this last approach that he gave the address 'In search of the engram', to which I have already referred at the beginning of this notice.

To many people, and sometimes to himself, it seemed that Lashley's scientific work was issuing, or had issued, principally in negations. Professor D. O. Hebb, for example, in a very fine review of his life and work, has written 'Lashley was left in the uncomfortable position of being against (existing) theory, a persistent critic of new as well as old ideas without having others of his own which he was willing to present as an adequate substitute'. 'But', he adds immediately, 'it does not do his theoretical position justice to regard his criticism as destructive only. He clarified many problems, often by setting them in a new context that his own research or his scholarship made possible, with corresponding implications for research.'†

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* The phrase generally used by Lashley and by others following him is 'mass action'.

† *Amer. J. Psychol.*, March, 1959, Vol. 72.

This is all true, but it is not all the truth. At the end of his address on the engram Lashley rather wryly said, 'This series of experiments has yielded a good bit of information about what and where the memory trace is not.' But everybody who heard him knew that, though the words may have been negative, he finished still full of the ardour of the search, still resolved that he, and others, must continue it, and that in spirit he was optimistic and positive. For anybody who looks there are, in this study as in very many others that he made, flashes of insight springing out of the level of scientific caution. 'It is highly probable that immediate memory is maintained by some sort of after-discharge of the originally excited neurones. Such can scarcely be the basis of more permanent memory.' No doubt others before him had made much the same suggestion but not with the same background or to the same purpose. Again, 'The serial timing of actions is among the most important and least studied of behavioural patterns.' And 'Skilled acts involve a timed series of actions which cannot be accounted for as a simple chain of conditioned reflexes.' We have to keep in mind that for him these suggestions, of memory, for example, as a skill, and of all skills as timed series of actions were not merely general notions gained by reflection. They were a summing up of a great mass of original, highly controlled observations; and since Lashley made them their definition and significance have increased.

Lashley possessed indeed, extreme scientific honesty and carefulness, combined both with outstanding technological and manipulative skill, and with a capacity for brilliant intuition. The first two, working in fields full of hastily framed hypotheses and theories, were bound to issue largely in destructive criticism. Scientific intuition, reaching forward towards discovery before the evidence which will compel everybody is known, is always preponderantly positive. It is beyond any possibility of dispute that in each of these ways Lashley's achievements will remain of permanent and outstanding significance. At the same time it is highly probable that many of his suggestions which could not at the time be fully substantiated, of the remarks which he seemed to make 'by the way' and of his flashes of insight will be found to have contributed most of all towards the solution of his problems.

Lashley had honorary degrees from the Universities of Pittsburgh, Chicago, Western Reserve, Pennsylvania and Hopkins. He was a member of the American National Academy of Sciences, of the American Philosophical Society and of the American Academy of Arts and Sciences. He served on the National Research Council (division of anthropology and psychology) in 1927-30, and 1932-5. He belonged also to the American Society of Zoologists, the American Physiological Society, the Society of Experimental Psychologists, and the American Society of Human Genetics. He was an Honorary Member of the American Neurological Association, the Harvey Society, the New York Academy of Sciences, the British Institute for the Study of Animal Behaviour, and Honorary Fellow of the British Psychological Society. In 1951 he was elected a Foreign Member of the Royal Society. He was awarded the Crosby Warren Medal by the Society of Experimental Psychologists, the

Daniel Giraud Medal for Zoology by the National Academy of Sciences and the William Baly Medal for Physiology by the Royal College of Physicians.

In 1954 Lashley fell seriously ill with a difficult anaemic condition. After prolonged and drastic treatment he seemed to have recovered and in 1957 he made, with his wife, his third and final trip to Alaska. The following year they came to England and to London, and on 28 July he signed the Charter Book of the Royal Society. They then went to France and, at Poitiers, on 7 August 1958, he died suddenly.

Lashley was twice married. His first marriage was to Miss Edith Ann Baker, in 1918. She was an excellent pianist, and he had taught himself to play the violin and the 'cello. Since 1920, he wrote later, he had 'devoted at least one evening a week to the playing of chamber music, of which I have collected an extensive library'. There is an authentic story that one evening at Minneapolis, a dozen or so people at the Lashleys' talked of how much a person would be willing to sacrifice for the sake of his research. One of the guests said, 'Karl, would you be willing never to hear a bar of music if, by giving it up, you could find out what you want to know?' He said, rather casually, 'Yes'. 'Not Beethoven!' his friend insisted. 'Yes', Karl replied, much less casually, 'I'd be willing to be deaf for the rest of my life.'*

His second great hobby was sailing, and his interest in this went far back to the days of his boyhood. At various times he owned a number of boats from a Star to a 36-foot yawl, and several of these contained original features which he had himself designed and constructed. He sometimes said that he would have liked best of all to venture out upon some long and lonely voyage across wide seas. It was partly his interest in boats that made him highly skilled with his hands. He thoroughly enjoyed designing and building apparatus and doing carpentry and cabinet making, and he was an accomplished chef. He liked chess and played it well, and it was, no doubt, due to an underlying whimsical sense of fun which never left him, that he got a lot of pleasure from nonsense poetry, invented limericks, and wrote masses of rhyming verse.

His first wife died in 1948, and in 1957 he married Claire H. Schiller, the widow of the distinguished psychologist, Dr Paul H. Schiller. It is of interest that almost the last considerable contribution that Lashley made to psychology was to write an introduction to a book on *Instinctive behaviour* which was translated and edited by his second wife. Paul Schiller had made some important observations on insightful behaviour in primates, and of this Lashley now wrote: 'The insight is the immediate one-trial learning to use the innate manipulative acts in the manner discovered by chance, and the generalization of the acts to other, similar situations . . .; an extension of the same concepts to the manipulation of ideas may well lead to the conclusion that man has failed to identify his own instincts because he calls them intelligence'.† In what sense, if at all, this is true may be decided in the future.

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* For this anecdote I am indebted to Mrs Lashley.

† Quoted from a memoir written by Dr Leonard Carmichael and published in the *American Philosophical Society Year Book for 1958.*

It shows that Lashley's flair for the intuitive flash remained active, and combined with his devotion to meticulous control to the end of his life.

In appearance Lashley was rather tall, and rather loosely built. He had a sensitive and expressive face, maybe a little sad in repose, but capable of lighting up into a most charming smile. Wherever he was present he could not be overlooked at the time, or soon forgotten. This was not, generally, because he talked a lot or pushed himself forward in any way; it was because he was one of those rare folks from whom a kind of excitement seems to spread out. Some found him shy, reserved and even stand-offish. Certainly he had little relish for the company of the unintelligent or the pretentious, and he did not love convention for convention's sake. But when he met a person, or faced an audience, whose intelligence and honesty could make some sort of a match with his own, he could be splendidly witty, gay and wise. No doubt there were ways in which rebels attracted him. At various stages he was much interested in J. B. Watson, in Pavlov, in Freud and in the Gestalt psychologists—all at odds against the respectable opinions of their day and clime. But he maintained his own integrity and was never swept away by any of them, for with a certain liking for intellectual risk and sweeping assertion he combined the scientist's loyalty to empirical observation and respect for experimental facts.

I wish gratefully to acknowledge the valuable assistance which I have received in the preparation and writing of this memoir from Mrs Lashley, and from a number of my American friends and Professor Lashley's colleagues. Mrs Lashley allowed me to use some notes written by her husband, and gave me much further information. She also provided the Bibliography. For the photograph and the signature I am indebted particularly to Professor E. G. Boring and to Harvard University, Dr Leonard Carmichael, Secretary of the Smithsonian Institution, and Professor Donald O. Hebb of the University of Montreal allowed me to make use of memoirs which they had written for the American Philosophical Society and the *American Journal of Psychology*.

F. C. BARTLETT

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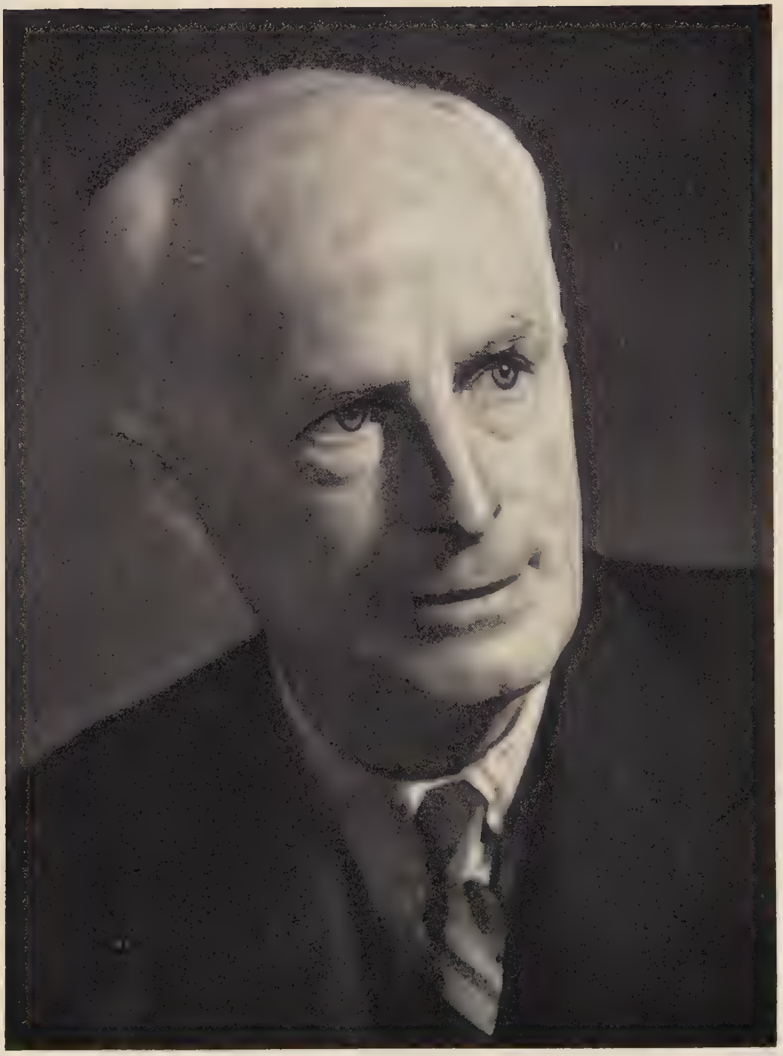

Douglas Mawson

DOUGLAS MAWSON

1882-1958

DOUGLAS MAWSON, Antarctic explorer, geologist and a leading scientific figure in the Australian Commonwealth, died in Adelaide on 15 October 1958 in his seventy-seventh year. He was born at Shipley, near Bradford, on 5 May 1882, the son of Robert Ellis Mawson and Margaret Ann Mawson of Otley, Yorkshire. With his parents he moved when very young to Australia and entered the University of Sydney as a student in mining engineering. He graduated B.E. in 1901 and was appointed to a demonstratorship in chemistry in the University in 1902. His early interest in scientific exploration was shown by his first expedition to the New Hebrides in 1903 where, under the auspices of Capt. E. G. Rason, R.N., he spent six months in geological investigations in almost virgin country. This proved no easy task and included dangers not only from hostile natives but from the inhospitable jungle as well.

During his years as a mining student in Sydney, Mawson had been greatly influenced by Professor T. W. Edgeworth David and after completing his assignment in the New Hebrides he returned to the geology school of the University from which he graduated B.Sc. in 1905. The early influence also of Professor A. Liversidge under whom he had served as demonstrator is reflected in Mawson's enduring interest in the chemical aspects of geology. He could be regarded as one of the Australian pioneers in the field of geochemistry. In 1905 he was appointed Lecturer in Mineralogy and Petrology in the University of Adelaide which he served in various capacities until his death in 1958.

Although on his arrival in South Australia he immediately started geological work with characteristic energy, this was soon interrupted. On the recommendation of Edgeworth David he was invited by Ernest Shackleton to join his expedition to Antarctica which sailed late in 1907 in the *Nimrod* from Lyttleton. Mawson's duties were those of physicist and he was chiefly concerned with geomagnetic and auroral studies. Winter quarters were set up at Cape Royds in the Ross Sea. Edgeworth David, who had accompanied the expedition as far as winter quarters and had intended to return in the *Nimrod,* eventually decided to stay in Antarctica over the winter and he and Mawson were associated in two notable achievements; one was the ascent and geological examination of the active volcanic cone of Mt Erebus, the other the attainment of the South Magnetic Pole which involved a man-hauled sledge journey over the Antarctic plateau of nearly thirteen hundred miles under the severest conditions.

Mawson's return to Adelaide in 1909 was quickly followed by preparations for further Antarctic exploration. These preparations were completed in 1911 when he led the Australasian Antarctic Expedition which sailed in the *Aurora* to set up its base in Adelie Land. But this was an expedition with aims entirely different from those of its forerunners. No attempt to reach the pole was made and the only objective was scientific discovery. The scientific rewards are generally considered to have far surpassed those of any former expedition. However these great results were not achieved easily. Adelie Land is probably the stormiest region of the whole Antarctic continent. In the course of an extensive sledging journey one of Mawson's companions and most of the party's food were lost in a crevasse and the other companion died of exhaustion and starvation. Only a man who had Mawson's magnificent courage, endurance and physique could have survived the appalling difficulties of the three-weeks' journey back to his base with enough food just to keep him alive. When he eventually reached his base it was to find that the *Aurora* had sailed for Australia only a few hours before, and Mawson and the party which had remained behind hoping for his return had to spend another winter in Antarctica. The expedition had taken with it the first air sledge and the first wireless installation to be established in Antarctica, and it was from the Adelie Land base in March 1913 that the dramatic message of Mawson's return from his ordeal was transmitted direct to Australia. It was typical of Mawson and the fine group of men in the expedition that there was no deterioration in the high quality of the scientific work during this second enforced year. The stream of scientific memoirs that were to be issued from the Australian Government Press through the succeeding forty years had their beginning in this enterprise brilliantly planned and executed in the face of exceptional difficulties. Mawson's general account of the expedition, published under the title of *The home of the blizzard*, has become one of the classics of polar exploration.

When war came in 1914 Mawson volunteered for army service, was appointed a staff officer and later attained the rank of major.

Further expeditions to Antarctica were made in 1929 and in 1931, when he led the British, Australian and New Zealand Antarctic Research Expedition to the regions south of Australia. A great contribution to our knowledge of the continent was made by means of a detailed geodetic and geological survey of the coast of the Australian sector of Antarctica. This gave the first delineation of the great region named by Mawson MacRobertson Land, Princess Elizabeth Land and the Mackenzie Sea. This was supplemented by important oceanographic and biological observations.

Although Mawson last visited Antarctica in 1931 it always retained his keen interest. He observed with enthusiasm all the developments of the International Geophysical Year. His profound knowledge has always been at the disposal of the Australian National Antarctic Research Expedition which for several years has maintained bases in the Australian sector, the main base being very appropriately named Mawson. Indeed, when Prince

Philip visited Australia in *Brittania* in 1956 it was Mawson who took the chair at the Antarctic symposium that was one important feature of the Melbourne programme, and the paper he delivered showed that his enthusiasm was as strong as ever and his intellect as keen.

A full account of Mawson's contribution to Antarctic research would perhaps suggest that he could not have devoted much time to geological work in Australia. A glance at the bibliography of his works will however immediately dispel any such idea.

When Mawson first took up his appointment as lecturer in mineralogy and petrology at the University of Adelaide he was quickly attracted by the great arc of Precambrian, highly-mineralized rocks which branch from the meridional Mt Lofty and Flinders Ranges and run through north-eastern South Australia into New South Wales. These rocks contain the noted Broken Hill ore-body and the region is a complex of igneous and sedimentary rocks which vary greatly in grade of metamorphism and degree of mineralization. Mawson was able to show that these rocks could be grouped into two divisions, an older group which he called the Willyama Series (from the aboriginal name for Broken Hill) and a younger Torrowangee Series. He also suggested that the older was probably Archaean, the younger Proterozoic in age. Later work supported by isotopic age-determinations has shown the soundness of his field observations and deductions. The results of this work are incorporated in a notable memoir on 'Geological investigations in the Broken Hill area' published in 1912. Another notable publication derived from the work of this period was 'Chiastolites from Bimbowrie, South Australia', which describes what must be one of the world's most remarkable occurrences of andalusite.

In the course of his examination of the newer Precambrian rocks of this region Mawson had observed notable occurrences of boulder-beds which he believed to be of glacial origin. This observation had a profound effect on his later work.

In 1906 a prospector brought some puzzling minerals to Mawson for identification. Mawson recognized the presence of uranium and later visited the locality of their occurrence, a place called Radium Hill, near Olary. From this locality in the same year he described as new, davidite, a radioactive mineral rich in titanium and carrying uranium. Though not immediately accepted as a homogeneous mineral, it has in recent years been fully established as a valid species. This was the first discovery of a notable deposit of uranium ores in Australia, a fact which became of much greater importance in years to come. Mawson thus acquired an interest in the minerals of the rarer elements, an interest he retained for the rest of his life. It was natural therefore that when uranium minerals were also found at Mt Painter in the north Flinders Ranges, Mawson should be consulted and that he should visit the locality. The Mt Painter area proved to be a mineralogist's treasure-house and attracted Mawson for many visits. These visits also showed him that in the area surrounding Mt Painter were wonderful exposures of sedimentary rocks similar to the younger Precambrian rocks in the Broken Hill region. Mawson's Antarctic experiences had given him a great interest in glaciology, and in the Flinders Ranges he again recognized glacial sediments which he later showed to be of extraordinary thickness and extent. It was in this way that Mawson began the work which was to absorb so much of his interest and activity for many years. Although at first he was mainly attracted by the evidence of glaciation it later became apparent that it was necessary to make a complete study of the stratigraphy of what is now known as the Adelaide System. One or two field trips were made to the Flinders Ranges nearly every year. Generally he took a small party of students with him. An immense area was covered in this work and many sections were carefully measured. At the time of writing, maps of part of the region are being prepared by the Geological Survey of South Australia. The Survey geologists pay the highest tribute to the accuracy and astuteness of Mawson's observations. His work was a great contribution to Proterozoic stratigraphy and to our knowledge of Precambrian glaciation. Mawson showed that glacial beds, in some places tillite but generally glacio-marine, extended for nearly a thousand miles in South Australia and that glacial conditions existed, intermittently, over an immense period of Proterozoic time.

Although Mawson's work on the stratigraphy and glaciology of the rocks of the Adelaide system must be regarded as his most important contribution to geological science his interests were extremely wide and his publications include such diverse subjects as the geological activities of algae, the geochemistry of South Australian igneous rocks and the origin of carbonaceous sediments. He had a great love of minerals and displayed the hall-mark of the great mineralogist—that uncanny ability of almost instantly recognizing that a mineral is unusual or 'new'. When Mawson first arrived in South Australia the geology of that large state, which has an area of about 380 000 square miles was known only in the most general way. Despite his frequent absences in Antarctica or on matters concerned with his expeditions or on military service he managed to visit most of the state and his knowledge of the whole large area was immense. He used all methods of travel, bicycle and motorcycle, horse-and-cart, camels in the arid regions and, later, motor-truck. Mawson never seemed happier than when he was on field trips. His usual good spirits were at their liveliest and he was a wonderful camping companion. He had a great knowledge of the Australian bush and easily made friends with lone prospectors and with the isolated families in remote sheep-station homesteads.

When in 1920 he was promoted to the chair of geology and mineralogy in Adelaide he completely revised the courses in his department. This gave him a very heavy load of teaching but was not allowed to curtail his field and laboratory work. Moreover it permitted him to take more students into the field. Practically all the students graduating from his department received their first experience of field work by accompanying him on one of his field camps in the Flinders or Barrier Ranges. To most it was an unforgettable experience and they will particularly remember how the professor did most of the cooking and made frequent brews of tea in a 'billy' in the manner of the Australian bushman.

It was natural that a man with Mawson's heroic record and striking appearance should appeal to students. But these things cannot account for the affection with which they all regarded him. His friendliness was one of his outstanding characteristics; he was one of the most approachable of professors whose enthusiasm for his subject had a supremely infectious quality. He could view the errors of inexperience with sympathy and tolerance but could be devastatingly frank in his criticism of what he considered foolish. Although Mawson's world-wide reputation was due to his physical achievements and exploration there is no doubt that he was a true academic to whom the welfare of his university and his science were the first consideration. His colleagues in the University were proud of the fact that although Mawson was offered attractive appointments in other countries he preferred to live and work in Adelaide. He was also much endeared to his colleagues not only by his extraordinary modesty but by his lusty sense of humour which also had a most infectious quality. His whereabouts in any gathering could often be judged by the concentration of shouts of laughter.

Honours came to Douglas Mawson from many countries and many institutions. As might be expected, the earliest of these (1909) came from the Royal Geographical Society in the form of its Antarctic Medal followed some years later by the award of the Founder's Medal. He also received the King's Polar Medal to which were later added two bars. The geographical societies of America, Chicago, Paris and Berlin also awarded medals. On his return from Antarctica in 1914 the honour of knighthood was conferred. In 1920 he received the order of the British Empire. From Italy came the Order of Saints Michael and Lazarus and the Order of the Crown of Italy.

The Geological Society of London awarded him the Bigsby Medal in 1919. He was elected Fellow of the Royal Society in 1923. The Von Mueller Memorial Medal was awarded by the Australian and New Zealand Association for the Advancement of Science in 1930 and the same Association elected him President for the period 1935-1937. In 1952 the University of Adelaide published in his honour the *Sir Douglas Mawson Anniversary Volume* in which were included geological papers from the professors of all other Australian, and some overseas universities, from members of the staff of his own department and prominent former students. When he retired from his chair in 1952 the University elected him Professor Emeritus and he remained a member of the Council of the University until his death. This year of his retirement saw also the completion of the building of the new Geology School at Adelaide. It was built from funds given in the first instance by the Commonwealth Government to the University of Adelaide for buildings after the second World War. This source was supplemented by a special grant from the South Australian Government which enabled the building to be completed on a generous scale. In naming it the Mawson Laboratory the Council of the

University departed from all precedent in that it was named after an active and distinguished member of the University staff rather than after the more usual type of benefactor. Mawson was one of the small group of foundation fellows of the Australian Academy of Science. The Geological Society of Australia elected him in its first election of Honorary Members in 1958. Honours came to him not only for his scientific work but also for his distinction as a great leader of men of science.

In 1914 Mawson married Miss Francisca Adriana (Paquita) Delprat, the daughter of Mr G. D. Delprat of Broken Hill and Melbourne. They had two daughters. For many years Sir Douglas and Lady Mawson, both of striking and engaging appearance, played extremely prominent parts in the affairs of the City and University of Adelaide. They entertained charmingly in their seaside home near Adelaide or took visitors for camping trips or picnics in the mountains. Lady Mawson frequently accompanied her husband on field trips and they shared a great love of trees and gardens.

The final honour to Douglas Mawson came after his death when by order of the Prime Minister of Australia he was accorded a State Funeral, an exceptional tribute reflecting the highest esteem of the Commonwealth for one of its most illustrious citizens, his achievements, his courage, his integrity and his humanity.

A. R. Alderman
C. E. Tilley

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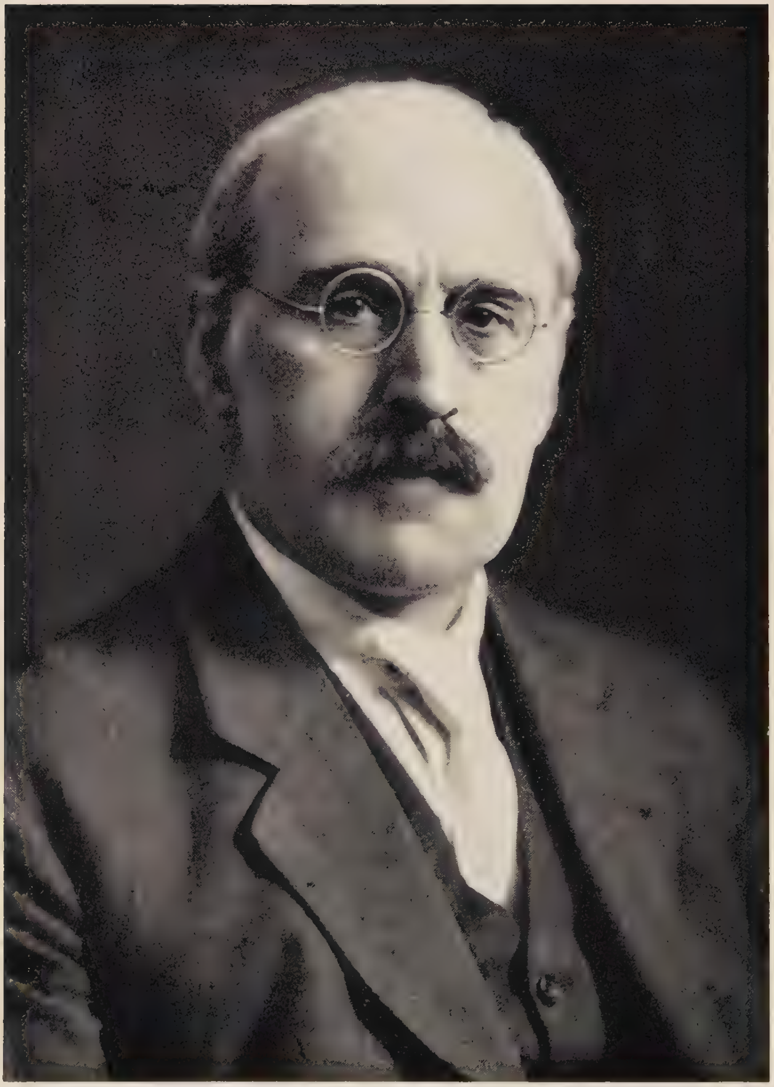

S. R. Milner

SAMUEL ROSLINGTON MILNER

1875-1958

S. R. Milner was born in Dodsworth, a village near Barnsley, Yorkshire, on 22 August 1875. His father, Samuel Wilkinson Milner, was an agent, or 'factor' for certain collieries in that district; he married a Miss Ann Roslington and founded a family of four daughters, and one son, the youngest child. During Milner's infancy the family removed to Retford on the borders of Sherwood Forest and in due time the boy was entered at the Retford Grammar School, a small but ancient Edward VIth foundation. Here, by good fortune, the headmaster, though irascible, was interested in science and able to take an interest in a boy of scientific tendencies. He awarded him a Headmaster's Prize of a microscope for he had become a keen naturalist. But Milner's mind soon turned to physical science, and the headmaster, seeing that the boy had already out-distanced all the others, provided him with physics textbooks and a quiet room where he could be alone and read and devour their contents. A science class of one pupil, without a master. He was not greatly interested in games but he was a powerful swimmer, and a great walker, exploring the forest, tree climbing and bird's-nesting. Indeed his interest in birds became so great that he built quite a large aviary in the back yard of his parents' home.

He won a Gilchrist scholarship to University College, Bristol, in 1892 and came under the care of Professor A. P. Chattock, an admirable teacher and lecturer. He read physics, passed the London Inter-B.Sc. and in most of the college examinations he headed the list. It is clear that he quickly showed signs of the intellectual capacity which he later developed, for in his Finals year, Professor Chattock, for whom he had a very high regard and affection, gave him a research problem to investigate. Within a very short time he proved himself to be a skilful manipulator, he quickly obtained preliminary results and suggested fresh lines of approach to the problem—which was the measurement of thermal conductivity. He was in fact considered to be, at 19 years of age, the most promising student the Department had had for many years. He graduated top of the list in the London B.Sc. in Experimental Physics in 1895, and was awarded a University Scholarship, followed immediately by an 1851 Exhibition scholarship in January 1896, tenable for the first year at Bristol, to determine the thermal conductivity of salt solutions. He soon displayed that feature which has characterized his life's work, the ability to do excellent experimental work yet make mathematical and theoretical contributions of the highest order. Two theoretical papers were written in his first year at Bristol. One was on the 'Variation of the dissociation coefficient of a binary electrolyte with temperature'—this was a more rigorous determination of the law derived theoretically by van t'Hoff, rigorous because it was confined to a binary electrolyte whereas van t'Hoff had investigated the general case of any chemical reaction and in so doing had had to make certain assumptions. The second paper on the 'Variation of the latent heat of vaporization with temperature' made allowance for the finite size of molecules in relation to their mean free path, and gave a formula in agreement with experimental results for water over a temperature range of 125 °C, and also gave an explanation of Trouton's Law connecting the latent heat of vaporization of different liquids with their boiling point (in degrees Kelvin). With Professor Chattock he continued the work he had begun as an undergraduate and devised an accurate method for the measurement of thermal conductivity of water, but the paper was not read for a year (to the British Association in 1898), by which time another investigator had produced almost the same results by an equally careful procedure.

The 1851 scholarship was granted for a second year, this time to be held at the Physical Chemical Institute at Göttingen to study under Professor Nernst 'the potential difference existing at the surface of mercury and electrolytes of different concentrations with reference to the theory of the Lippman capillary electrometer'. The object of the research was to test a theory advanced by Nernst that the potential difference was caused by the osmotic pressure of the mercury ions in the solution. Milner proved the theory qualitatively, but the results were not sufficiently definite to give quantitative support. An excellent report on this work is to be found in the library of the 1851 Commissioners.

He said he had less time for experimental research in Göttingen because there was so much to learn from a great man and the time was well spent. He became fluent in German and so for the rest of his life he was versed in German scientific literature, and believed that all graduates intending to take up science as a career ought to have a good working knowledge of German as well as of French. To that end he instituted in the Honours Degree course in Sheffield, when he was Professor of Physics, a rule that two questions in the final examination would be printed, one in German and the other in French, and replies to both questions would be obligatory. The questions were usually fairly long statements on some scientific bone of contention and the student would be invited to comment on this bone, thus being challenged to display his linguistic and forensic abilities. Above all, however, the year with Nernst vitalized his interest in electrolytic theory to which he made lasting contributions over the next twenty years.

He returned to England in the autumn of 1898 to an appointment on the staff of the University of Manchester as a demonstrator in physics. An incident in connexion with this appointment shows Milner's wonderful powers of assimilation. On his way to Manchester he passed through London and bought a copy of J. J. Thomson's *Conduction of electricity through gases* on the day of its first publication. It so interested him that he spent most of the night reading it, completing it in the train the next day. Professor Schuster interviewed him, mentioning the book of which he had received an advance copy, and was incredulous when the young applicant claimed to have read it. Questions soon showed the truth of his assertion and Milner was appointed.

We have no knowledge of his activities over the years 1898 to 1900 except that he submitted a thesis to the University of London for the degree of the D.Sc. which was granted in 1899, and wrote a note on the 'Theory of solution pressure', the theory already mentioned due to Nernst. This note examined the exact nature of the assumptions made in deriving the contact difference of potential between a metal and an electrolyte containing the metal ion in solution, and showed that the formula could be derived for completely dissociated salts solely by cyclical application of the Second Law; the same argument dealt with incomplete dissociation and also the effect of the presence of any other salts in solution, a problem to which an answer had not previously been available.

From Manchester he moved to Sheffield in 1900 to join Professor Hicks as his sole lecturer at the University College in the Department of Physics, and here he remained until his retirement, being promoted to full lecturer in 1905, Secretary to the Faculty of Pure Science from 1911 to 1914 and holding the Chair for nearly twenty years. Hicks was the Principal of the University College and, when Sheffield received its Charter in 1905, became its first Vice-Chancellor, so Milner carried a large load of teaching responsibility for many years. That he was able to do so much original research is testimony to his great ability and immense capacity for hard work. Nor did he skimp his teaching duties—far from it; he lectured on the whole range of physics included in the university curriculum of those days. His lectures were lucid and well illustrated by experiment, for he was a keen experimenter with a very practical bent, and the standard he set up was a challenge to all the staff who served under him in later years. Moreover he welcomed informal discussion after his lectures and was never too busy to unravel difficulties or misunderstandings. He was very interested in his students and until the end of his days at Sheffield he made a point of giving at least one course of lectures to first-year students so that he might get to know them, and they him. This accounts for the fact that all students of the University in science and many in medicine remember with gratitude the good start they had on the road in physics, and none forgot 'the Tiger'; why he was called 'the Tiger' has never been clearly explained for no one can recall evidence of sharp claws. He did however at times give the appearance of being quite cross for no apparent reason! Dr W. F. G. Swann who joined the Physics Department in 1907 as Assistant Lecturer recalls that Milner was the first person he met on arrival at the University, and his first impression was rather alarming. 'I thought he looked quite cross and my heart sank within me. I was later to learn how erroneous one's first impressions can be, for, as it turned out, never have I had to deal with a more kindly and considerate individual than I subsequently found him to be.'

Hicks was interested in spectroscopy and Milner became acquainted with, and soon expert in, vacuum problems of those days. Two papers in 1903 are doubtless the product of irksome wastes of time from which many research workers suffer; leaking vacuum stop-cocks have been known to generations of post-graduates—and are still with us—but Milner's new form with a mercury seal is the perfect answer except that it is complicated. It held a 'Crookes dark-space vacuum' for a week, a creditable performance even for modern taps. Töpler pumps were the order of the day and older physicists will recall the boredom of evacuating a flask with a Töpler; Milner's automatic pump (British Patent specification No. 493, 1903) evacuated a 250 ml. flask to an X-ray vacuum in 14 minutes. The paper shows the application of his clear mind to the essential needs of a good pump. It is interesting to note that even as a student in Bristol he had been alert to the use of the latest techniques to solve his problems; for example, he had used an X-ray tube in 1897 to exhibit the soundness of electrical connexions inside a junction box! In the next few years in Sheffield he devised new methods of measuring resistances and capacitances in electrolytic studies, and he introduced at a very early date a cathode-ray tube and a rotating mirror to study the development of spark discharges. The method he introduced for measuring combined resistances and capacitances arose out of the lack of sensitivity and precision of the old induction coil/telephone method of measuring capacitances. Milner adapted Ayrton & Perry's secohmmeter—(two commutators on the same shaft to reverse both the battery and the galvanometer terminals of a Wheatstone's bridge system)—to the measurement of resistance of an electrolyte, thus avoiding polarization, to the measurement of surface capacitance of electrodes in a solution the resistivity of which is not negligible, and to the measurement of the capacitance of a leaky condenser: in all these measurements the equivalent resistance and capacitance cannot be taken apart and measured separately. Nernst had shown that with the telephone the two adjustments of two variables could be made independently, but Milner showed how the secohmmeter could be more effectively applied, and as in all his work, the practical conclusion was fully supported by mathematical proof. This apparatus was used in his experimental verification of Nernst's theory of polarization in electrolytes to which reference is made later.

He gave an explanation of the mechanism by which solutions of soap and some organic substances, such as acetic acid, are able to form durable liquid films. In pure liquids where the surface tension is uniform lasting films cannot be formed so some mechanism whereby the surface tension alters with film thickness must be operative for solutions which produce films. Milner considered the thermodynamic relations between the surface tension and the concentration of the dissolved substance in the surface, and showed that the surface tension will vary with concentration only if the osmotic pressure can vary with area. For this to be the case he suggested that in the surface film which is the seat of the capillary forces, the concentration of solute is different from what it is in the interior of the solution and as the film expands, solute migrates from the interior to the surface, so reducing the osmotic pressure: the surface acquires a 'surface excess'. As the osmotic pressure can be derived from the lowering of the freezing point of the solution the magnitude of the 'surface excess' can be calculated. It is found to be negative with inorganic salt solutions, thus accounting for their inability to form thin films, and positive for the organic solutions examined—positive and independent of the initial concentration of solute in solution. There is an apparent paradox for soap solutions which have a surface tension independent of concentration and Milner attempted an explanation of this paradox. We are indebted to Professor N. K. Adam, F.R.S., for some comments on this paper: he draws our attention to the fact that Milner's neat proof of the relation between 'surface excess' and $d\tau/dc$ had been more generally derived by Gibbs thirty years earlier, but as the paper was only to be found in an obscure Connecticut journal it is assumed that Milner was unaware of it; he further states that the soap solution paradox was not fully explained for many years because the laws of dilute solutions do not hold for soap solutions on account of the formation of ionic micelles or clusters of long-chain molecular ions.

It was probably when he was setting up some laboratory optical experiments on Newton's fringes that he saw a new system of fringes which had hitherto escaped notice, even in such an old subject as this. If two pieces of plate glass are closely spaced and show Newton's fringes from light reflected at both sides of the air film between them, another system of fringes may be seen under the right conditions, and Milner traced these fringes to reflexions from the remote faces of the two sheets of glass meeting again at the air interface. By using a second piece of plate glass cut from a large sheet of plate glass therefore, it is possible quickly to survey the large piece for variations of thickness, a very practical and simple method. An optics expert tells us that he is not aware that this method for the examination of plate glass has been used; it appears to have escaped notice by the specialists.

His contribution to the spectroscopic work of the Department was the identification of three long series of spectrum lines of mercury, the principal, the diffuse and the sharp series up to $m=16$ for the first two and $m=14$ for the last, and he presented these results to the British Association meeting in Sheffield in 1910 when he acted as the local secretary.

Milner used a rotating mirror to study the growth of streamers in an electric spark, an interest which again was probably due to the preoccupations of the Department with spectroscopic research under Hicks's influence, and to the fact that Schuster in Manchester, at the time when Milner was there, had first used a rotating mirror placed between a spectroscope and a photographic plate to study the development of metal vapour lines in a spark discharge. Milner extended Schuster's work by examining streamers in the spark when the circuit contained inductance and capacitance, whereas Schuster's work had been to study the spark in a purely resistive circuit. The axis of the mirror was placed at 45° to the plane of the spectroscope so that the photographs showed the history of the growth and decay of each spectrum line without confusion. He showed unambiguously that metal vapour evaporated more abundantly from the cathode than from the anode, presumably due to the higher temperature of the cathode, and that the metal vapour is charged negatively since its drift to the anode is reversed when the potential of the oscillatory circuit reverses. Arc spectra in general last longer than spark spectra, but, as one would expect, the relative duration of the different modes of excitation depend on the inductance in circuit. It was to study the current/voltage characteristics of the spark circuit that he set up in 1911 a home-made cathode-ray oscillograph, and a good oscillograph it must have been. A willemite screen on a plain sheet of glass covered the end of the tube, and by placing photographic plates in direct contact with this screen he recorded oscillations of 2×10^8 cycles per second! He unravelled rather complicated i/V records and traced out in detail the sequence of the glow-to-arc transition (from 300 V down to 35 V drop). He showed moreover that if the discharge occurs at atmospheric pressure in a narrow-bore tube, the transition to arc does not occur—a forerunner of the 'expulsion tube' of modern electrical engineering practice—and he gave reasons based on the current/voltage characteristics of the discharge for his earlier observations on spectra emitted at the cathode, the anode and from the core of the discharge.

About this time, Milner became affected with sciatica and it was doubtful whether he would be able to continue active work. He went on a long sea voyage and returned with very little improvement. Then he tried X-ray treatments. The result was, apparently, quite beneficial and he remarked more or less facetiously that he intended to pay his debt to X-rays by investigating them. He built a home-made X-ray tube, and assembled into it a target comprising eight different metal elements placed side by side, together with means for moving them across the tube so that they could be bombarded in turn by a beam of electrons with a view to discovering the 'hardness' of the characteristic X-radiation emitted by each element. He was building ionization chambers to study the absorption coefficients of the X-rays when Mosley published his classic work on this subject and Milner abandoned the work without further study.

Interspersed with these experimental essays he extended his interest in the theory of electrolytes and laid the foundation of the modern theory. His approach was essentially that of a physicist who used mathematics to yield an understanding of the phenomena. The Nernst theory of the back electromotive force set up by polarization in an electrolytic cell was known to be in excellent qualitative agreement with what was then known about polarization, but no direct quantitative comparison of experimental results with theory had been made. Milner calculated the amount of polarization which developed with an arbitrarily varied current, and then measured the corresponding degree of polarization in various solutions, obtaining quite good agreement between theory and experiment. Three papers on electrolysis followed in quick succession (except for a gap of four years of World War I). The osmotic pressure of dilute solutions of inorganic electrolytes, as determined from the lowering of their freezing point, is nearly always but not quite so great as it would be if the dissociation of the electrolyte were complete, and Arrhenius had suggested that the difference is due to an appreciable fraction of the molecules remaining undissociated; but this theory gives results at variance with the law of mass action. If ions in solution, instead of obeying the law for perfect gases, behave differently because of the *electrical* forces existing between them, the theory will no longer hold. Milner first worked out in a paper, 'The virial of a mixture of ions' (1912), the influence of all the ions in an electrolyte on one another, the distribution of ions being modified because like ions repel one another, and unlike ions attract one another: the result of this attraction and repulsion is that on the average, instead of a perfectly random distribution, ions of opposite sign will be slightly nearer together, and those of like sign on the average will be slightly further apart. This problem turned out to be an enormously complicated operation with the computational facilities of those days but the work became regarded with the highest esteem. The mathematical computation of this virial was applied in the second paper, 'Effect of interionic forces on the osmotic pressure of electrolytes', to the determination of the osmotic pressure, which is found, in consequence, to be lower than that given by the gas law, and therefore diminishes the amount by which the freezing point is lowered. This computation was based on the assumption that dissociation is complete. The theoretical values of the lowering of the freezing point of potassium chloride and sodium chloride solutions agreed exceedingly well with the experimental results over a 20:1 range in concentration of the ions. Not only were the slopes of the curves in good agreement, but the absolute value of the lowering was correct. A further derivation of the slope of a curve for an electrolyte in which dissociation is assumed to be incomplete was found to depart so much from experiment that this lent support to the idea that dissociation *must* be complete. With several other solutions it was found that there were some small differences between theory and experiment, but the differences were in the opposite direction to what one would expect if only partial dissociation had occurred, so this gave extra grounds for confidence in the theory of complete dissociation. Milner must have worked out the effect of these inter-ionic forces on conductivity and known that the effect was also in good accord with experiment, for he writes confidently of this in the 1913 paper. However his War service intervened, and the two papers embracing the results on the influence of inter-ionic forces on conductivity were not written until the autumn of 1917. Briefly stated he proved that the inter-ionic forces which reduce osmotic pressure of free ions in an electrolyte, produce the same reduction in the average velocity with which the ions move in carrying a current. He summarized his general theory of inter-ionic forces at the Faraday Society Discussion in January 1919, and whether he would have continued this work is a matter for speculation, for he suffered a serious illness and Debye, taking up this work, went ahead so fast that Milner turned his attention to electro-magnetic theory. Debye's 1923 paper in the *Physikalische Zeitschrift* pays high tribute to Milner's work.

We get a glimpse of the difficult days before the war—when Milner was engaged on these various researches—in a letter from Dr Swann who was Assistant Lecturer for a few years: 'When in these days, I think of the galaxy of secretaries, duplicating machines, telephones, and all of the multitudinous means of wasting time, it is with some nostalgia that I recall that while we were at Sheffield we acquired a telephone in the department so that it was unnecessary to go to another part of the University if we wished to telephone. Of course there was no secretary, nor even a departmental typewriter, but Milner bought one of his own. He longed to have some computing aid in the form of a simple calculating machine but the funds of the Department did not seem able to stand such a monetary outlay, and Milner bought one himself. The budget for all apparatus for instruction and research amounted to £150 a year, but, fortunately, Milner and I were good glassblowers so we got along pretty well with our limited resources.'

Milner's academic career was interrupted in 1914 by his eagerness to assist in the war effort. He was not able to do active soldiering but he joined the R.A.M.C. as a private at the suggestion of a university colleague, Col. A. M. Connell, who was Commandant of the Third Northern General Hospital, a Territorial unit which opened a base hospital in Sheffield. In a small room he built an X-ray apparatus after the way of a physicist, designed a couch and screening apparatus which incorporated protective devices far in advance of the times, and with these he did some extremely efficient work. He trained scientific colleagues who went to other hospitals as radiographers and received his reward by promotion to the rank of quartermaster sergeant. Neither pay nor rank mattered to him however: he was happy in the knowledge that he was of service. His dress was not all soldier-like because there was always difficulty with his puttees, but no one paid much regard to this, all admired his modest and whole-hearted willingness to be available at all hours of day or night. Perhaps the only rebuke he received was when one of his former students was taken into the X-ray room on a stretcher: Milner promptly welcomed him with outstretched hand, only to be pulled up by a sergeant because the stretcher case was wearing the pips of a captain! Later, the patient was able to help occasionally in the X-ray room and willingly accepted instructions from the N.C.O.

In 1917 Professor Hicks retired from the Chair of Physics and Milner was appointed 'Acting Professor for the duration of the war or such later time as might be determined'. It has been written that 'it is difficult to see now why this distinguished physicist was not appointed outright. Perhaps his great natural modesty stood in the way of others recognizing his merits.' It must be remembered that his health was far from good and there was some doubt as to whether he would be strong enough to stand the strain of office. He at once returned to his work on electrolytes and published the last two papers on this subject in February and May 1918. In 1918 he became a member of the Physical Society and in 1920 he joined the newly-founded Institute of Physics. His appointment to the Chair was confirmed in 1921 and in 1922 he was elected to the Royal Society.

We have no record of the reason why Milner turned his attention to electromagnetic theory at this stage. As with his work on electrolytic theory his first approach was that of a physicist rather than of a mathematician considering the motion and stresses in electro-magnetic lines and tubes, and in this way building up an analysis of the electro-magnetic field into moving elements, before his thoughts matured into more mathematical considerations of the 'action' of an electro-magnetic field on which his later work was based. The first of these papers 'Internal energy of the Lorentz electron' pointed the need to ascribe to an electron a certain amount of energy in addition to that which is accounted for by the electric and magnetic forces of its self-field. The need arises from a consideration of the Poynting flux of energy in the field of a uniformly moving electron and from this can be deduced 'a value of the internal energy of the electron which is in complete accord with that necessitated by the mechanical theory.' Then he tried to answer the question 'does an accelerated electron *necessarily* radiate energy on the classical theory?', apologizing for raising such a question after the quantum theory had brought such revolutionary changes. He thought he saw a way by which, with a minor modification to the boundary conditions, it was sufficient to do away with radiation in at least one case of accelerated motion: but Professor Born drew Milner's attention to the fact that his proof was implicit in a relativistic treatment of the problem which Born himself had made early in 1909.

In the third paper of this series 'Electromagnetic lines and tubes', he discussed a paper by Professor Whittaker in which the conception of tubes of electrostatic force and magnetic force is extended to the general electromagnetic field. Milner tried to present a clearer picture of what these tubes are, and deduced that the general magnetic field, when it is considered as a four-dimensional entity, appears as the exact and natural extension to four dimensions of the three-dimensional magnetic field. Then he showed that 'the stress system of a four-dimensional electro-magnetic field' forms another respect in which a similar exact equivalence holds with an electrostatic field in three dimensions. After a lapse of time he returned to the problem of quantizing the electro-magnetic field ('An analysis of the electromagnetic field into moving elements'), first by translating into terms of space and time properties of the four-dimensional electro-magnetic tubes which had been derived in his two previous papers; then by proceeding direct from Maxwell's equations of the electro-magnetic field and, making certain transformations, he showed that small moving elements of the electro-magnetic field can be constructed, which can be regarded as keeping their identities permanently as they move, and he showed that they exhibit also rotational movement. Such field elements in space-time are elements of action. This appeared revolutionary; that from Maxwell's equations of the microscopic field, finite and discrete field elements could be deduced. In his paper on 'The "Action" on an electromagnetic field' Milner expressed the properties of the field by making use of his conception that e and h are quantities *in motion* in the observer's space, instead of taking the more usual view that they vary with the time *at a fixed point* in that space. He was able to show that all the theorems of mechanics applicable to a moving fluid subject to stress hold, when properly interpreted, for such a moving electromagnetic field. He applied the principle of least action to the study of identifiable field elements, space-time elements with action included, moving along certain paths and with certain velocities. Thus he derived expressions for the Hamiltonian and Eulerian *actions* of the field which form strict equivalents to those for the *action* of a dynamical system.

Then follows a series of remarkable papers, two before retirement, 1933 and 1936, on the foundation of the electron wave equation, and two in his retirement, on the relations of Eddington's E-numbers to the tensor calculus, to which must be added much more material now being sorted out by Professor Gabor with a view to publication. It seemed incredible to his younger friends, colleagues and former students that at 80 years of age he could continue to pour out extremely advanced mathematics with the vigour of a young man. Professor Gabor, who was a close neighbour of Professor Milner in his retirement in Rugby from 1940 onwards, shared many of Milner's thoughts during this period and himself became fascinated by the Professor's ideas. He has kindly written a special note on this phase of his life.

Note by Professor D. Gabor, F.R.S.

The last 25 years of Milner's life were devoted to a great intellectual adventure, inspired by the ideas of Sir Arthur Eddington. It so happened that Eddington was one of Milner's first students when he was a young demonstrator at Owen's College. After this they hardly ever met in their life, and it appears that Eddington never knew that his erstwhile demonstrator had become his most faithful pupil, because Milner, with characteristic shyness, did not want to disclose his work even to Eddington until it was finished to his own satisfaction—which it never was!

Eddington's thesis was that 'An intelligence, unacquainted with our universe, but acquainted with the system of thought by which the human mind interprets to itself the contents of its sensory experience, should be able to attain all the knowledge of physics that we have attained by experiment'.* In other words, the laws of physics are the laws of thought. This was probably the boldest solution any scientist had ever proposed for the riddle of the 'intelligibility of the universe'. Einstein's opinion was that we shall never understand it. Dirac, more confidently, said that, however crazy the world, we could always find an algebra to fit it. This is, more or less, the faith by which most theoretical physicists live. Eddington's faith was of a different nature; he believed that the only algebra possible was that of his E-numbers.

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* A. Eddington, *Relativity theory of protons and electrons*. Cambridge Univ. Press, 1936, p. 327.

This was the natural filter through which the human mind breaks the crude data of experience, hence the laws of nature could be nothing but the identities of this algebra.

Milner never shared Eddington's philosophical convictions, but he recognized the great driving power of his mathematical methods, and devoted 25 years to their elucidation. This was greatly needed, because Eddington's E-number calculus was so obscure that a distinguished theoretical physicist has not unjustifiably called it a 'smoke screen'. Once the smoke screen was cleared off it revealed a new mathematical tool of unexpected interest, which led Milner to remarkable discoveries in classical relativistic physics. Unfortunately to this day only his mathematical investigations have been published (Milner 1952, I and II), while his physical researches, on which he kept polishing and improving until the time of his death, are still awaiting publication.

Eddington's E-numbers are linear compositions of 16 square roots of -1, E_μ, with numerical (real or complex) coefficients. The E_μ commute or anticommute according to a rule so complex that only Eddington could handle them, and it can be doubted whether even he could avoid mistakes. G. Lemaître first introduced a certain measure of order into them, by ordering the E_μ in a 4-square matrix which Milner calls the E'-matrix. After repeated transformations Milner found the simplest representation, in terms of his '$\mathcal{E}$-matrix', which is a 4-square matrix of 16 four-square matrices, all real :

$$
\begin{array}{c|cccc|cccc|cccc|cccc|c}
\beta \longrightarrow & \multicolumn{4}{c}{1} & \multicolumn{4}{c}{2} & \multicolumn{4}{c}{3} & \multicolumn{4}{c}{4} & \\
\hline
\alpha & 1 & \cdot & \cdot & \cdot & \cdot & 1 & \cdot & \cdot & \cdot & \cdot & 1 & \cdot & \cdot & \cdot & \cdot & 1 & \\
\downarrow & \cdot & 1 & \cdot & \cdot & -1 & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & 1 & \cdot & \cdot & -1 & \cdot & \bar{R} \\
1 & \cdot & \cdot & 1 & \cdot & \cdot & \cdot & \cdot & -1 & -1 & \cdot & \cdot & \cdot & \cdot & 1 & \cdot & \cdot & \\
 & \cdot & \cdot & \cdot & 1 & \cdot & \cdot & 1 & \cdot & \cdot & -1 & \cdot & \cdot & -1 & \cdot & \cdot & \cdot & \\
\hline
 & \cdot & -1 & \cdot & \cdot & 1 & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & -1 & \cdot & \cdot & 1 & \cdot & \bar{S} \\
 & 1 & \cdot & \cdot & \cdot & \cdot & 1 & \cdot & \cdot & \cdot & \cdot & 1 & \cdot & \cdot & \cdot & \cdot & 1 & \\
2 & \cdot & \cdot & \cdot & -1 & \cdot & \cdot & -1 & \cdot & \cdot & 1 & \cdot & \cdot & 1 & \cdot & \cdot & \cdot & \\
 & \cdot & \cdot & 1 & \cdot & \cdot & \cdot & \cdot & -1 & -1 & \cdot & \cdot & \cdot & \cdot & 1 & \cdot & \cdot & \\
\hline
\mathcal{E}= & \cdot & \cdot & -1 & \cdot & \cdot & \cdot & \cdot & 1 & 1 & \cdot & \cdot & \cdot & \cdot & -1 & \cdot & \cdot & \bar{T} \\
 & \cdot & \cdot & \cdot & 1 & \cdot & \cdot & 1 & \cdot & \cdot & -1 & \cdot & \cdot & -1 & \cdot & \cdot & \cdot & \\
3 & 1 & \cdot & \cdot & \cdot & \cdot & 1 & \cdot & \cdot & \cdot & \cdot & 1 & \cdot & \cdot & \cdot & \cdot & 1 & \\
 & \cdot & -1 & \cdot & \cdot & 1 & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & -1 & \cdot & \cdot & 1 & \cdot & \\
\hline
 & \cdot & \cdot & \cdot & -1 & \cdot & \cdot & -1 & \cdot & \cdot & 1 & \cdot & \cdot & 1 & \cdot & \cdot & \cdot & \\
 & \cdot & \cdot & -1 & \cdot & \cdot & \cdot & \cdot & 1 & 1 & \cdot & \cdot & \cdot & \cdot & -1 & \cdot & \cdot & \\
4 & \cdot & 1 & \cdot & \cdot & -1 & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & 1 & \cdot & \cdot & -1 & \cdot & \bar{U} \\
 & 1 & \cdot & \cdot & \cdot & \cdot & 1 & \cdot & \cdot & \cdot & \cdot & 1 & \cdot & \cdot & \cdot & \cdot & 1 & \\
\hline
 & \multicolumn{4}{c}{R} & \multicolumn{4}{c}{S} & \multicolumn{4}{c}{T} & \multicolumn{4}{c}{U} &
\end{array}
$$

This matrix has many interesting properties. The columns, R, S, T, U, and the rows $\bar{R}$, $\bar{S}$, $\bar{T}$, $\bar{U}$ are all quaternionic sets, that is to say they multiply like l, i, j, k. They have powerful *selector* properties, because they allow expressing the products of *any* quaternionic set of four 4^2-matrices as linear expressions with coefficients $R \ldots U$. For instance, let F_α ($\alpha = 1 \ldots 4$) be a quaternionic set, then the product matrix term of any two of these is

$$F_\alpha F_\beta = T_{j\alpha\beta} F_j$$

where the tensor summation rule has been used. From this point on all Milner's results can be written *formally* as tensor relations, but with an important qualification regarding their content. As he puts it '. . . matrices have useful features which pure symbols cannot have. They possess an *internal structure*, which not only gives them greater power of representation, but allows them to be modified by *transpositions*.'

It can be said that it is at this point that Milner started on a new path, not even suspected by Eddington. He first showed that from any matrix A there can be derived another, ${}^eA$, which he calls an *e-number* or the *e-transpose* of A by the operation

$${}^eA = \tfrac{1}{2} A_{\alpha\beta} \mathscr{E}_{\alpha\beta}$$

or, explicitly,

$${}^eA = \tfrac{1}{2}\begin{bmatrix} (A_{11}+A_{22}+A_{33}+A_{44}) & (A_{12}-A_{21}-A_{34}+A_{43}) & (A_{13}+A_{24}-A_{31}-A_{42}) & (A_{14}-A_{23}+A_{32}-A_{41}) \\ (-A_{12}+A_{21}-A_{34}+A_{43}) & (A_{11}+A_{22}-A_{33}-A_{44}) & (-A_{14}+A_{23}+A_{32}-A_{41}) & (A_{13}+A_{24}+A_{31}+A_{42}) \\ (-A_{13}+A_{24}+A_{31}-A_{42}) & (A_{14}+A_{23}+A_{33}+A_{41}) & (A_{11}-A_{22}+A_{33}-A_{44}) & (-A_{12}-A_{21}+A_{34}+A_{43}) \\ (-A_{14}-A_{23}+A_{32}+A_{41}) & (-A_{13}+A_{24}-A_{31}+A_{42}) & (A_{12}+A_{21}+A_{34}+A_{43}) & (A_{11}-A_{22}-A_{33}+A_{44}) \end{bmatrix}$$

Two *e*-transpositions in succession restore the original matrix A.

Alternative, and somewhat simpler expressions of this transformation are

$${}^eA = \tfrac{1}{2} A_{\alpha\beta} R_\alpha S_\beta = \tfrac{1}{2} S_j A R_j$$

In this transformation from A to ${}^eA$ the sum of squares of the terms, or its square root which Milner calls the *resultant magnitude* has remained unchanged

$$({}^eA_{ik}\,{}^eA_{ik})^{\frac{1}{2}} = (A_{\alpha\beta} A_{\alpha\beta})^{\frac{1}{2}}$$

Milner next considered the transformations which the *e*-transpose of a matrix undergoes if the matrix itself undergoes a standard transformation of the type

$$A \rightarrow A' = PAQ$$

and found the following remarkable result. *If two matrices are in the relation of e-transposes to one another, and if either matrix is subjected to one of the transformations* (in which K and M are arbitrary 4^2 matrices)

$$K_{\kappa\lambda} R_\kappa S_\lambda (\ ) M_{\mu\nu} R_\mu S_\nu \qquad (a)$$

$$K_{\kappa\lambda} R_\kappa S_\mu^{-1} (\ ) M_{\mu\nu} R_\lambda^{-1} S_\nu \qquad (b)$$

then the other matrix will undergo the other transformation. (R^{-1}, S^{-1} result from R, S by interchange of rows and columns.)

There is a considerable difference between (a) and (b). (a) is a transformation of the standard type, but (b) is not, because the λ- and μ-suffixes have become transferred to the wrong side of the operand, hence the sides do not represent single matrices. The complete transform is in the general case the sum of 256 elementary 'permutation' transforms.

These *Milner transformations*, as they now may be called, have the *group property*, that is to say any two consecutive transformations can be replaced by a single one of the same type, and they have the '*tensor property*', that is to say, equations of the type

$$\Sigma({}^{e}A)_{ik} = 0$$

are not affected by them.

It is a rather moot point whether Milner has, in fact, discovered, as he believed, a new class of *tensors*. One can equally well contend that pure symbols which transform by the rule (b) can be rightly called a new class of tensors, or that they are equivalent representations, 'images' of ordinary tensors and their transformations.* A real difference arises only by the *physical identification* of the symbols. Milner gives a first illustration of this by a geometrical interpretation.

Consider a 4-square matrix produced by outer multiplication of a column 'vector' a and a row 'vector' $\bar{b}$. We can consider the matrix $a\bar{b}$ as representing these two vectors. But its e-transpose is, as Milner shows, a matrix whose columns (or rows) represent four mutually perpendicular vectors, all of equal length, equal to the square root of the scalar product of a and $\bar{b}$. Neither is more 'fundamental' than the other, both are constructed out of the same raw material of eight data. We can as well identify the space of $a\bar{b}$ as the space of ${}^{e}(a\bar{b})$ with physical space, or we could perhaps, as Milner suggests, locate the mechanical effects in one, the electrical effects in the other. There is one powerful hint in this last of Milner's papers published before his death that such an attempt may be worth while. If the vector pair undergoes a rotation in the first space (a rotation if this is a real four-space, a Lorentz transformation if it is space-time), the corresponding transformation in the second space appears as the composition, in arbitrary order, of two 'locked' rotations of the R- and S-type. In this sense one can say that Milner had succeeded in decomposing the Lorentz transformation into two more elementary transformations. We know the heuristic power of the Lorentz transformation in selecting the possible laws of physics. Is it not possible that the more elementary transformations will reveal something which has (in Darwin's words, quoted by Eddington and by Milner) 'slipped through the net'?

Milner conceived a comprehensive plan for the application of his new mathematical concepts to physics, which he called 'The field theory of matter'. Twelve chapters of this are left in his legacy, but only the first two relating to electromagnetic theory and the theory of the electron, were ready for publication in 1954. Even these were not published until his death, owing to several unfortunate circumstances, chief of which was his excessive shyness and self-criticism.

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* C. W. Kilmister (*Proc. Roy. Soc.* A, **218**, 144, 1953) has shown that the Milner transformations are included in the modern tensor theory which is co-extensive with matrix theory.

In his electromagnetic theory the leading mathematical idea is to use a matrix and its e-transpose *together* in one analysis, so that they represent two physical features of electromagnetism. The geometrical illustration suggests using a matrix constructed of two vectors, representing perhaps the two field vectors. But if there is to be correspondence between a matrix and its e-transpose invariant to rotations, the matrix must be such as to have an invariant 'resultant magnitude'. (Invariant sum of squares of matrix elements.) This condition is satisfied if the two vectors in question are *complex conjugates*. Milner therefore puts for the two vectors

$$\mathbf{z} = \mathbf{e}+\mathrm{i}\mathbf{h} \qquad \mathbf{z}^* = \mathbf{e}-\mathrm{i}\mathbf{h}$$

and finds

$$\mathcal{Z} = {}^{e}(zz^*) = \tfrac{1}{2}(e+\mathrm{i}h)R(e-\mathrm{i}h)S = {}^{t}\mathcal{Z}+{}^{r}\mathcal{Z}$$

The part ${}^{r}\mathcal{Z}$ turns out to be identical with the Maxwell energy-momentum-stress tensor of electromagnetic theory, if the vectors $\mathbf{e}=e_x,\ e_y,\ e_z$ and $\mathbf{h}=h_x,\ h_y,\ h_z$ are identified with the electric and magnetic vectors. This suggests that the term ${}^{t}\mathcal{Z}$ which is novel contains just that part of the energy-momentum-stress tensor which is missing in classical electromagnetic theory, and by its absence causes its well-known inconsistencies. This new term is

$${}^{t}\mathcal{Z}=\begin{bmatrix} \tfrac{1}{2}(e_t^2+h_t^2) & -\mathrm{i}(e_te_x+h_th_x) & -\mathrm{i}(e_te_y+h_th_y) & -\mathrm{i}(e_te_z+h_th_z) \\ \mathrm{i}(e_te_x+h_th_x) & \tfrac{1}{2}(e_t^2+h_t^2) & e_th_z-e_zh_t & -(e_th_y-e_yh_t) \\ \mathrm{i}(e_te_y+h_th_y) & -(e_th_z-e_zh_t) & \tfrac{1}{2}(e_t^2+h_t^2) & e_th_x-e_xh_t \\ \mathrm{i}(e_te_z+h_th_z) & e_th_y-e_zh_t & -(e_th_x-e_xh_t) & \tfrac{1}{2}(e_t^2+h_t^2) \end{bmatrix}$$

This part, and only this one contains the 'time components' e_t and h_t of the electromagnetic vectors, which are absent in standard theory.

From the energy-momentum tensor $\mathcal{Z}$ Milner derives, by applying the action principle, the 'extended' electromagnetic equations. These are the only invariant field equations compatible with $\mathcal{Z}$, and Milner therefore considers them as *identities* of his e-calculus. In vector form

$$\operatorname{div}\mathbf{e} = j_t = r_t-\frac{1}{c}\frac{\partial e_t}{\partial t} \qquad \frac{1}{c}\frac{\partial \mathbf{e}}{\partial t}-\operatorname{curl}\mathbf{h} = -\mathbf{j} = \mathbf{r}-\operatorname{grad} e_t$$

$$\operatorname{div}\mathbf{h} = k = s_t-\frac{1}{c}\frac{\partial h_t}{\partial t} \qquad \frac{1}{c}\frac{\partial \mathbf{h}}{\partial t}+\operatorname{curl}\mathbf{e} = -\mathbf{k} = \mathbf{s}-\operatorname{grad} h_t$$

These contain magnetic charges k and currents $\mathbf{k}$, which must be suppressed as usual. What is left differs from the standard equation only in the right-hand sides of the first two equations, in the charge-current vector. That is to say there is a difference only *inside the electron*, where no measurements can be made. Milner notes that these equations could have been derived in a trivial way, merely by giving the four-potential complex instead of real components. The superiority of Milner's approach reveals itself only in the further analysis, which must be summarized very briefly.

It is well known that classical electromagnetic theory is not self-consistent, because the electron would not hold together; it requires forces of a non-electromagnetic type to make it behave as a particle. Milner shows that his additional terms are *just those which are required to make an extended classical electron stable and relativistic.* There is in fact a somewhat embarrassing wealth of possible solutions in Milner's theory, because the 'locked rotations' R and S allow creating a great variety of electron models which would behave identically in classical experiments. In particular a great variety of rotational motions can be imposed on the fluid composing Milner's electron, without making any difference to its dynamics in outer fields.

Thus Milner has at last succeeded in fulfilling H. A. Lorentz's programme of a self-consistent electrodynamics extending into the inside of the electron. Of course, nobody nowadays considers this in itself as a reasonable physical quest. Milner was fully aware that his work would justify itself only if his theory could be quantized. It is not unreasonable to believe that the difficulties of modern quantum field theories have their cause in the inconsistencies of their classical parent theories. This belief is not exactly popular among theoretical physicists, but it has Dirac as one of its supporters. Quantization of Milner's theory could proceed on well-tried lines, because it can be completely translated into Hamiltonian form. Is it too much to hope, as Milner hoped, that some young theoretical physicist would extend his work into the quantum field, into which he, whose academic education in physics was complete by 1900, never dared to venture?

Finally one might ask whether Milner's work has given any support to Eddington's philosophical views? The answer is that, if anything, it has proved the opposite, that mathematics, even restricted by relativity, is so flexible that it can construct an infinity of consistent worlds, and there can never be any guarantee that even one of these will be isomorphic with the real one!

As his good friend for the last 20 years of his life, I should dearly love to see Milner rising to posthumous fame—at a time when 100 million dollars will be devoted to the experimental study of the 'extended' electron and proton. But at what astronomical distance is this vast modern team-physics from his! When I think of dear old Milner sitting in his study in Bilton, after some hours' work among his flowers and bees, comfortably explaining the latest progress of his theory, which always progressed and was never finished, with the most perfect professor's wife preparing the tea with honey in the next room, I realize that it would not matter so much if his physics were of the stuff dreams are made of. It is great stuff and a beautiful dream!

Professor W. E. Curtis, F.R.S., also writes 'Milner was one of the finest all-round physicists equally interested and proficient in every branch, such as very few, if any, could claim to be today. I particularly admired his scholarship questions which he set in the Final Honours papers because although they were elementary they could not be answered without understanding and thought. They were clear evidence of Milner's profound grasp of physics and of his original and lively mind.' As External Examiner for many years to the Universities of London and Leeds he carried out his duties with scrupulous care and painstaking conscientiousness so that no examinee ever suffered from less consideration than he deserved.

So we come to his closing years. He retired from the Chair in 1940 and was made Emeritus Professor; two years later he was awarded an honorary degree of D.Sc. Sheffield. The Milners set up their new home near to their son in Rugby where he and Mrs Milner enjoyed the pleasure of seeing their grandchildren growing up. He took up bee-keeping and applied scientific methods to it to keep the temperature of the beehives constant. With his wife he made many journeys by car to relatives and friends all over the country, whilst his home was always open and a wonderful welcome awaited any of his pupils who stopped by. The Golden Wedding was celebrated in September 1951 with a gathering of old students and staff around them, but in 1952 his son was appointed to the Chair of Applied Physics at the New South Wales University of Technology at Sydney, and they faced one of the biggest problems of their lives. They did not go to Sydney then but after two years they went out for a cruise to Australia, coming home determined to go back again to settle in Sydney. Severe illness overtook Milner as the ship neared England in the middle of winter, and this nearly cost him his life, but he recovered and two years later was well enough to face the journey once more, this for the last time. Again their friends gathered around to bid them God Speed, but on the very night before sailing, when it was literally too late to turn back, the Professor's serious illness with dropsy was recognized and only modern medicine and the brilliant administrations of the ship's doctor saved his life whilst at sea. They joined their son and his family in Sydney in May 1957.

One of the writers (J.R.C.) went with them on this journey and witnessed his amazing recovery from such an illness. The other writer (T.E.A.) had the great pleasure of visiting the Milners in June 1958. Although Milner had had yet another long illness he was recovering his strength and moved with some difficulty around the room, but was full of questions about science in England, about his friends, and about old times, and then suddenly he poured out his latest thoughts on tensor mathematics leaving his listener dazed. There seemed every reason to believe that he would live for a year or two longer to write up the work of these last few years which had been so much interrupted by periods of ill-health, but he died suddenly on 12 August 1958, at the age of 82.

In his early life at Bristol his interests were wide. With the lady who became his wife, Miss Winifred Walker, a graduate in physics of Bristol University College and herself an 1851 Exhibitioner, he founded the Students Union by amalgamation of the Men's and Women's Clubs of which they were Presidents. They were married shortly after his appointment to Sheffield University College and throughout his life he received the greatest possible support from his wife in matters academic and social. They both took a keen interest in his students, inviting them to their very cheerful home, and Mrs Milner's hospitality helped to thaw the atmosphere which in those days surrounded the head of the department perhaps more than it does today.

In years of great financial difficulty for Sheffield University when it was impossible to start any men's Halls of Residence, the Milners contributed practically and most generously to the welfare of the students by purchasing a house adjacent to their own. They furnished it, installed a housekeeper and for eight years conducted it as a hostel for men.

In his spare time he tramped many miles of the wild moorlands around Sheffield with his camera and tripod on his back; he extended his range by pedal cycling and was an early rider of the motor cycle at the beginning of the century. Motoring remained one of his chief pleasures to the end of his days and before leaving finally for Australia at the age of 81 he made a long circuit by car to see friends and relatives throughout England.

All his students and fellow members of staff had a tremendous admiration for his encyclopaedic knowledge of physics which enabled him to discuss their problems profitably as well as sympathetically. This was of very special value to his Honours students for in their final undergraduate year he set each a small research problem to tackle—as indeed he had been so treated by Professor Chattock—and guided them wisely in their first steps in research. He also expected each to give a few lectures in that same year to the assembled staff and research students, and then offered kindly advice on subject matter and manner of delivery. It must be admitted that his conversation at times was disconcerting because of his concern that words should be given their precise meaning, and in these lectures his Honours Students would suffer frequently by the disturbing effect of his interventions. It was not sarcasm or a desire to score off the unwary, but the involuntary reaction of a searching intelligence—as when a doctor remarked that his speedy recovery from an illness was incredible, and he received the reply 'Doctor, nothing that has been observed to happen can be incredible.' Milner was ahead of his time in recognizing in the years before World War I the industrial need for good physicists in the development of its materials and techniques and products. He took the University Physical Society to many local industrial companies, principally steel, of course, so that students might gain an insight into manufacturing processes and encouraged his graduates to enter British industry.

He was not administratively-minded and disliked committee work, preferring to give what help he could in quiet consultation. Perhaps it was because he intervened so infrequently in debates in the Faculty and Senate that when he did speak, he had a great influence on the proceedings. But it was not only in academic problems that his advice was sought, gently given and found helpful by all. His outstanding characteristic was his capacity for inspiring affection. He was the friend of all, because he was so willing to be friendly and helpful.

We are grateful to a number of friends who have written to us: to his widow and son in Australia, Dr C. F. Bareford, Major J. W. Buckley, Sir James Chadwick, F.R.S., Dr A. W. Chapman, Dr J. D. Craggs, Professor W. E. Curtis, F.R.S., Professor J. Kenner, F.R.S., Dr C. Sykes, F.R.S., Professor W. F. G. Swann, Professor R. Whiddington, F.R.S., and the 1851 Commissioners.

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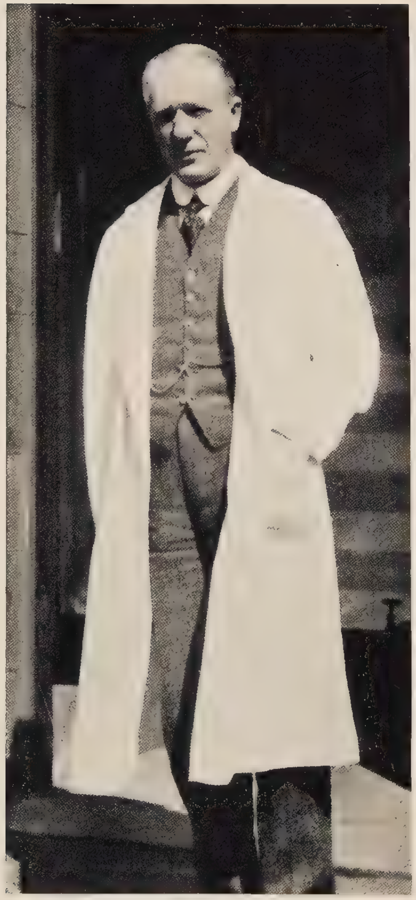

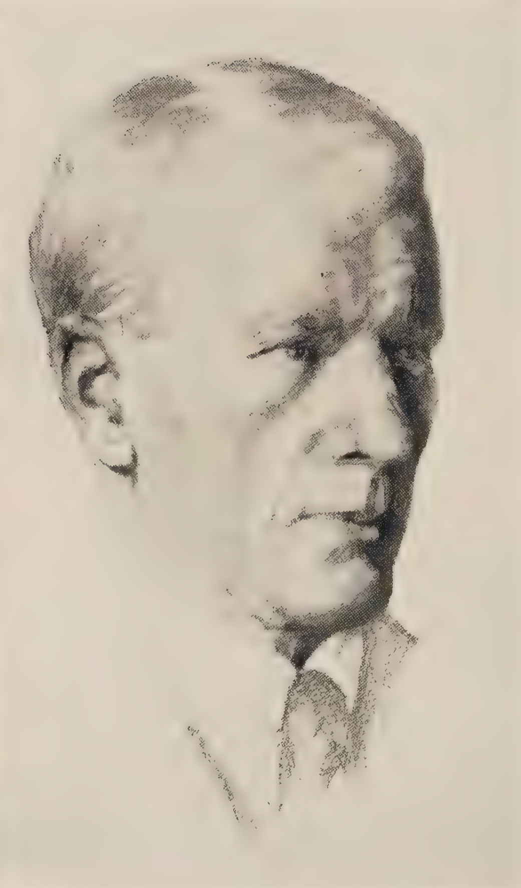

Robert Muir

ROBERT MUIR

1864-1959

If Thomas Carlyle's dictum be sound that 'Sincerity, a deep, great, genuine sincerity, is the first characteristic of all men in any way heroic', then, indeed, Robert Muir was a man of heroic stature. 'A silent great soul, he was one of those who cannot but be in earnest, whom Nature herself has appointed to be sincere.' This is the testimony of many of Muir's intimate friends who were privileged to penetrate his armour of decent pride and modest reserve and learned that the key to his life was unrelenting honesty of purpose.

Early life and family

Robert Muir was born on 5 July 1864, at Balfour, Stirlingshire, Scotland. He himself records that his parents belonged to the ordinary middle class and that there was nothing of importance in his ancestry. A grandfather is said to have been a Presbyterian minister. His father, the Rev. Robert Muir, M.A., soon after Robert's birth became Minister of Allar's U.P. Church, Hawick. 'My father . . . had a small stipend and no other means. This means that there was nothing in the way of luxury in the household, far from it; at the same time there was no actual hardship. I was brought up in a religious and intellectual atmosphere, at least as far as family life was concerned; outside in the town there was little of either quality. My father was a man of saintly character and very liberal minded for that time, nevertheless the restrictions then common in manses were little to my liking, especially was this the case with regard to Sunday observance. I had no difficulty in taking a good place in my classes at school but my interests were chiefly in sports and games and my chief pleasure was being in the open air. My father died when I was 18 years of age and our family then left Hawick and went to live in Edinburgh. It was a hard struggle then for us all, and during my University course I had to undertake a considerable amount of private tuition.' These candid sentences from the personal record that Sir Robert prepared for the Royal Society about 15 or 16 years before he died are illuminating. Very little is known about his mother, Susan Cameron Duncan, daughter of Wm Duncan, a merchant in Dundee. He has left no indication of his feelings towards her, nor of the place she occupied in the little family of five. Robert was the second child. Four sisters, one his senior, grew up to be women of high accomplishments. The eldest married David A. Welsh, M.D., who became Professor of Pathology in the University of Sydney. He was known as 'Taffy' Welsh to many generations of students who attributed to him an uncanny knowledge of pony racing. The second girl wrote short stories, the third was a distinguished classical student and the first woman to take the degree of M.A. with first-class Honours in Classics at Edinburgh University. Anne Davidson Muir, the youngest, turned out to be an artist of distinction, using water-colour as her principal medium. She was a Member of the Royal Scottish Society of Painters in Water-Colours from 1916 to 1951. Her subjects were usually flower compositions although she painted small portraits, mainly of the family. Miss Muir exhibited regularly in the best known Scottish exhibitions. The Art Gallery of Glasgow possesses two of her flowerpieces in oils and one in watercolour. Robert, who remained unmarried, seldom spoke about his family background; indeed, all such matters were treated with dignified reticence that forbade intrusion even by close friends. The Muirs rarely displayed emotion although Robert could unbend and his warm heart might even melt the icy despair of those who summoned up courage to seek his help. Pride that is far removed from arrogance must be respected. Quite properly, the Muirs preferred to be judged by their accomplishments and reserved for their own small circle the intimacies and judgements of family life.

Schooling and University

Sir Robert's personal narrative continues: 'I entered Hawick High School at the age of six and continued there till its closure some six years later. I then went to Teviot Grove Academy, a private school, where I remained till I was fifteen. I then spent a year in study at home under my father's direction. I gained many prizes at the Academy and was dux on two successive years. The teaching at school was excellent in various ways but was deficient as a preparation for the University. I cannot say that any of my teachers were men of eminence; I owed most to my father so far as higher education was concerned.

'At the age of sixteen I gained the Sir Walter Scott bursary in Latin, Greek and mathematics, an entrance bursary to the Arts Curriculum in Edinburgh University. This bursary was instituted by the Border Counties Association and was awarded every four years to boys from the border counties. I entered Edinburgh University at the age of 16 and studied the subjects then compulsory in the arts curriculum, viz. Latin, Greek, mathematics, natural philosophy, logic and metaphysics, moral philosophy, and English literature. I have found this training to be one of great value and influence. I was prizeman in mathematics and in natural philosophy.'

In 1884, at the age of 20, young Muir commenced his medical studies and he graduated in 1888. Despite a heavy burden of coaching whereby the family's precarious finances might be bolstered up, his record was excellent. He gained the Grierson Bursary twice for the subjects of the first and second years of study respectively. Class lists record firsts in anatomy, with a class mark of 83 in 1st year anatomy, in practical physiology with a mark of 77, in general pathology and practical pathology with top place. At the end of the third year he was awarded the Vans Dunlop Research Scholarship, at that time the most important scholarship in the curriculum. Later on he acknowledged how much he had been influenced by Professors G. Chrystal and G. Guthrie Tait in arts, and in medicine by Professors W. S. Greenfield and Y. R. Fraser. He graduated Master of Arts in 1884 and M.B., C.M., with First Class Honours, in 1888. Greenfield's influence must, indeed, have been tenacious for young Robert Muir promptly elected to enter the Department of Pathology which was directed by that uncommon man.

Postgraduate training

'For two years after graduation in medicine I conducted research, chiefly on diseases of the blood. During 6 months of this time (in 1889) I acted as Resident Physician with Professor Greenfield in Edinburgh Royal Infirmary.

'In 1892 I was appointed Demonstrator of Pathology in Edinburgh University and held the post till 1898. For a year I was also a Tutor in Clinical Medicine. In 1894 I was elected to the newly-instituted Lectureship on Pathological Bacteriology'. Such is the bald epitome of ten years' experience in teaching and the everyday duties of a pathologist and clinician, for the two aspects of medicine were at no time separated. Muir worked in the closest association with Greenfield whose grip on the unfolding mind of his brilliant young assistant is clearly discernible in the researches the latter embarked upon.

William Smith Greenfield was born at Salisbury in 1846 and educated at University College, London. After graduating Doctor of Medicine in 1874 he held several important posts in London, first as a Demonstrator in Morbid Anatomy at St Thomas's Hospital, London, and later as Professor of Pathology at the Brown Institution. He was called to the Chair of Pathology at Edinburgh in 1881 and held that post until 1912. He had written extensively on such subjects as visceral syphilis, lymphadenoma—his description of this condition is a classic far ahead of its time—pyaemia and renal disease. At the Brown Institution he worked out the bacteriology of woolsorter's disease, which is a variety of anthrax. Greenfield's authority at Edinburgh was considerable and his course in pathology stood supreme in the United Kingdom. He it was who insisted that medical students should be trained in practical pathological histology and it was to his assistant German Sims Woodhead, afterwards Professor of Pathology at Cambridge and founder of the *Journal of Pathology and Bacteriology*, that he assigned the task of planning such instruction. The value of this move cannot be over-estimated since the methods of pathological histology enable the student to probe, in a vivid fashion, the empiricisms that he acquires in the autopsy room and at the bedside. Greenfield was also one of the Professors of Clinical Medicine at Edinburgh, with charge of wards in the Royal Infirmary. No-one would know better than he the need to blend the harvest gleaned from the living subject with that reaped through the investigation of diseased tissues at operation or after death. He, no doubt, was responsible for young Robert Muir giving up some of his time to histology and physiology and acting as one of the University

Tutors of Clinical Medicine in the Royal Infirmary. At Greenfield's bidding, too, Muir added instruction in elementary bacteriology to his other duties as Senior Assistant in 1892 and for several years he acted as Examiner in Physiology for the Licence of the Royal College of Physicians and Surgeons of Edinburgh, which shows the esteem in which his knowledge of physiology was held. To Greenfield he appeared to be 'a man of wide knowledge, sound and balanced judgment, and of great care and circumspection; a careful and accurate observer, and withal a capable exponent of his researches, he has already gained for himself and his work a wide recognition, especially in relation to the pathology of diseases affecting the blood.' Above all, Greenfield applauded his invaluable faculty of getting others to work and of winning their co-operation. This 'modest, strong and capable man, a universal favourite with the students' as he was described in 1899 by John Chiene, President of the Royal College of Surgeons, was obviously marked out for great things. Not the least of his services, and one that carried his name far and wide, was the publication, with Dr James Ritchie, at the time Lecturer in Pathology to the University of Oxford, of the *Manual of bacteriology* in 1897. Illustrated by Richard Muir, laboratory assistant to the Pathology Department, this remarkable handbook has passed through eleven editions over the years and stands unchallenged in its clarity and scholarship. It combines workaday laboratory information and clinical acumen so astutely that the reader almost unwittingly absorbs the principles of bacteriology.

Dundee and Glasgow

In 1898, at the early age of 34, Robert Muir was called to the newly-instituted Chair of Pathology in the University of St Andrews. The classes were held in Dundee. Despite the heavy claim on his time consequent upon initiation and development of this new enterprise he prosecuted his researches with enthusiasm and insight that boded well for the future. The fame of the Pathology Department in the growing Medical School seemed assured under Muir's guidance when the death of Professor Joseph Coats in 1899 led to his choice as obvious successor in the Chair of Pathology at Glasgow. This gave him a modest degree of prosperity since his salary was 800 golden sovereigns at a time when a knickerbocker suit cost around 3 guineas, boots less than a guinea and a golf club 5s. 6d. From then onwards, for 36 years, he worked unceasingly for the welfare of the Glasgow School of Medicine and the Western Infirmary. Under skilful guidance the School of Pathology achieved a world-wide fame and his pupils became known for their thorough grounding in pathology and bacteriology, an appreciation of the results of recent research and their ability to apply this knowledge to clinical problems. They were proud of Muir's observational and experimental contributions that were illuminating many aspects of pathology; they recognized in him a pioneer in bacteriology and immunology and they profited by his textbooks, for in 1924 the *Textbook of pathology* joined the *Manual of bacteriology* and at once settled down to a long successful run. An astonishing number of his assistants, many of whom he had taught at Glasgow, achieved high academic office in Great Britain and throughout the Empire. He was to write a few years after his retirement in 1936: 'I have been specially fortunate in having as co-workers a considerable number of men who have taken distinguished places in pathology. In Scotland eight Chairs are at present occupied by such men, viz. in Glasgow, C. H. Browning (Bacteriology), D. F. Cappell (Pathology), J. W. S. Blacklock (Pathology), later of St Bartholomew's Hospital, London, J. W. McNee (Medicine); in Edinburgh, T. J. Mackie (Bacteriology); in Aberdeen, John S. Young (Pathology), John Cruickshank (Bacteriology); in St Andrews, Alan C. Lendrum (Pathology). In England, the following occupants of professorial Chairs have been my students: in Leeds, Matthew J. Stewart (Pathology) and Walter J. McLeod (Bacteriology); in London, Postgraduate Medical School, J. H. Dible (Pathology); in Birmingham, G. Haswell Wilson (Pathology); in Newcastle upon Tyne, E. M. Dunlop (Bacteriology). There are also others who are now dead.'

He chose men with a distinct capacity for research and teaching and he encouraged them to mark out a line of work for themselves. Of the undoubted success of his foresight there can be no doubt, but it was his honesty and fairness that brought him the trust of his pupils and colleagues and laid up for him an inexhaustible fund of affection that sustained him in his old age. And his University colleagues quickly sensed his integrity and finely-poised judgement. Muir was appointed to the University Court and served on innumerable committees both in Glasgow and elsewhere. During the war of 1914-18 he held the rank of Lt.-Colonel R.A.M.C.(T.F.) and was in charge of the pathological and bacteriological work of the 3rd and 4th Scottish General Hospitals. He also acted as Inspector of Laboratories in Scotland throughout those grim days. He served on the two committees for investigation of foot and mouth disease, being Chairman of the first, on the Medical Research Council from 1928-32, the Councils of the Imperial Cancer Research Fund and the British Empire Cancer Campaign. He assisted many deserving causes outside the University, including the Cancer Hospital and the Western Asylum's Research Institute.

Many distinctions came his way. He was elected a Fellow of the Royal Society in 1911, was a member of its Council in 1926-7 and was awarded a Royal Medal for his work on immunity in 1929. Honorary degrees were conferred upon him by the Universities of Bristol, Dublin, Durham, Edinburgh, Glasgow and Leeds, and he was made an Honorary Fellow of the Royal Colleges of Physicians of London and Edinburgh, of the Royal Faculty of Physicians and Surgeons of Glasgow and of the Royal Society of Medicine, and an Honorary Member of the Pathological Society of Great Britain and Northern Ireland. In 1933 the Canadian Medical Association invited him to give the Fourth Lister Oration and in 1936 the Royal College of Surgeons of England awarded him the Lister Medal. In 1931 many of his pupils and friends presented him with his portrait painted by G. Fiddes Watt, R.S.A. —it now hangs in the University—and at the same time a bust by G. H.

Paulin, A.R.S.A., was given to the University, who handed it over to the Western Infirmary to be placed in the Pathological Institute there. On that occasion, Sir Frederick Gowland Hopkins, President of the Royal Society, who made the presentation, said that it was Muir's accessibility, his sympathy, and his scientific generosity which first of all had created an atmosphere in his department which allowed his scientific vision and knowledge to have full play. Professor Muir had created in Glasgow a centre of pathological thought second to none in this country; he had been 'a torch-bearer for many years, who had illuminated the paths of progress for British pathologists.' He was knighted in 1934.

Muir retired when he was 73, working actively even up to the last day. Settling in his beloved Edinburgh, he quietly gave himself up to many scientific interests, especially botany and geology, punctuated with fishing, golf and bridge. The writer of this memoir recalls a long dissertation on the volcanoes of Scotland that entranced him one evening in an Edinburgh club. Perhaps the paramount interest was the doings of his old pupils and he was always ready with the proper word of advice and, indeed, of criticism when the occasion warranted it. Many pathologists will recall with deep affection his all-too-infrequent appearances at the Pathological Society when his friends conspired to draw him into a discussion and encouraged him to break the rules that rigidly controlled the procedure of that august body.

Despite a set of illnesses that would have subdued most people, this lion-hearted old man went on fishing some Highland stream or loch and playing his favourite golf almost to the end. Not so many years before his last illness he flew to Australia to see his eldest sister. Both of them were then near to 90. Death came peacefully and with dignity in his 95th year. Many of his old pupils and friends, with representatives of the Universities and Medical Societies of Scotland, attended the last, simple ceremony at Warriston.

Personality

A host of recollections that have been generously placed at the writer's disposal by Muir's personal friends and assistants may be allowed to speak for themselves.

One who joined him in 1911 writes: 'Muir at that time would be in his late forties, a well-built, very handsome man. He used to say that his facial beauty was spoiled by an incident at golf in his younger days when a golf ball rebounded from a bunker backed by railway sleepers, and smote him on the nose, breaking it. I think it was the famous Redan hole at North Berwick (? 14th hole—I have often played it and looked at the sleepers)!

'When I entered Muir's department, various more senior men were there before me. J. H. Teacher, soon to go over to the Royal Infirmary as Professor, Carl Browning, not long back from his time with Ehrlich, and "full of" the new Wassermann reaction, then John Shaw Dunn, shortly afterwards lecturer to Teacher and Haswell Wilson, later Professor of Pathology at Birmingham. At this time Muir lectured 5 days a week on both pathology and bacteriology. He was in the post mortem room every morning at 10 to 10.30 a.m. and always did at least one autopsy with his own hands. The medical chiefs and their classes always came over at 11 a.m. and Muir discussed the case. The surgeons, being engaged in operating, often could not come, but generally sent over an assistant. It was all tolerably formal and an apology was sent over by a chief if he could not come.

'When I arrived he told me I had to learn the technique of routine pathology and bacteriology as well as doing my research work. He had a problem for me at once, so I fortunately got no choice. I did the donkey work—Muir provided the brains, and the training. One of his rules, and a good one, was that no worker in his laboratory dare ask a technician (there were only two of them) to cut or stain a section until he had been two years at least in the Department, and the same applied to bacteriological technique. It was a hard but excellent discipline.

'At that time Muir's main hobby was golf. He played a fair game, not very good and never bad. Every Saturday morning he, and three of his friends, left by an early train for Prestwick, the championship golf club in the West. His chief friends were Ralph Stockman, Professor of Materia Medica, MacNeile Dixon, a well-known Shakespearean scholar and writer, and Gloag, an authority on Scottish law. Occasionally one failed to turn up and then one of us would be asked to come, a great joy.

'Muir was a bachelor and lived with two of his sisters; one of them, older than himself, was the housewife, the younger, Anne, was a water-colour painter of much skill who exhibited at all the big shows in Scotland. She had a real "artistic temperament" which in later life let her down and she died sadly in a private home. She was Robert's favourite, and he missed her terribly.

'In the department he was always very approachable at any time of day, and I can see him now looking down his rather poor single-barrel, triple nosepiece microscope, discussing my sections, with a cigarette hanging out of the corner of his mouth and screwing up his eyes to avoid the smoke. He was practically a chain smoker of cigarettes for many years; he also smoked a pipe, especially when out-of-doors. For years he wore a long black alpaca coat in the department and even in the post mortem room, with a waterproof apron over it. Those long black coats seemed to be the hall-mark of the pathologist in those days!'

Another assistant recalls that 'It was away back in 1913 that, as a student taking the class of practical pathology, I first met him. He gave half-an-hour's talk in the lecture theatre at each meeting of the class before we went upstairs to the laboratories for the practical work, under Shaw Dunn or Haswell Wilson, who were the Lecturers at that time. "Bobby" himself, and the department, were characterized by a welcome absence of all irksome formality, and by a friendly tolerance of reasonable expression of undergraduate ebullition! The peculiar quality of his pronunciation of the letters "r" and "s" was an attractive novelty which we sought diligently to reproduce! (I hear even now his "progressive, purposeless, proliferation" when speaking of tumour growth.) Bobby's lectures were consistently well attended in my time; and, as attendances were taken only occasionally, this was of itself a good indication that we found his lectures well worth listening to. He used notes only sparingly, and gave the impression of being fully primed with—and absorbed in—what he was speaking about. I remember on one occasion when he had used some word that was new to us, and hadn't written it up on the blackboard, there was much shuffling of feet—the customary way of drawing a lecturer's attention—but this didn't seem to get through to him at all, and he paid no attention to it. The shuffling continued, and ultimately he stopped and inquired somewhat brusquely of the class "What is it?" After several attempts we got across to him what we wanted, and he immediately turned round and wrote the word for us. Then he faced the class again, and there rolled from him "There is no need to make an abominable noise like that. The slightest indication of any reasonable request will be immediately acceded to." It had taken a very considerable effort on our part to get the indication through to him! It didn't seem to irritate him in any way, but rather to disconcert him. We liked him enormously.

'I was a student demonstrator in the practical class the following summer (1914); and each demonstrator was expected, and encouraged, to undertake some "research" in his spare time. The supervision of this was delegated to one or other of the lecturers but "the Chief" kept *au fait* with what the demonstrators were doing. During this spell there was a student in the class who was much older than the others (he had been a school teacher before he started medicine) and who was a strange creature. He invited ragging, and he was ragged almost mercilessly. One warm afternoon someone had poured canada balsam on the stool on which he subsequently sat during the hour-and-a-half meeting of the laboratory class. As a result, when he rose at the end of the class the stool came with him. He got separated from it, and dashed downstairs in a towering rage to complain to Bobby. Through the open communicating door between the Chief's room and the one in which I was, I heard what passed. The aggrieved and angry student poured out his tale of woe—drew a harrowing verbal picture of how he would have to stand in the Underground on the way home to the other side of the city—of how his trousers were ruined for all time, and demanded compensation for himself, and condign punishment for those responsible for the outrage. He talked fifteen to the dozen for several minutes, during which there wasn't a sound from Bobby. Then the rapid fire talk ceased, and was followed by dead silence for perhaps ten seconds. Then Bobby's voice, "Shocking waste of good balsam!" That was all.

'From the latter part of August 1914, till October 1915, I served as what might rightly be described as a technician at a Territorial R.A.M.C. Hospital. Bobby—a Lt.-Col. R.A.M.C.(T.F.)—was pathologist to the hospital, and visited it as often as was required. His understanding helpfulness made things easy for me, and he was ever willing to do things himself so as to show me how to do them. For the first blood cultures that were asked for, and the first requests for Wassermann tests, he himself went to the wards with me and took the specimens. Quite early he got me to co-operate with him in investigating streptococcal infection of war wounds, and thus introduced me to research based on material coming to the laboratory in the way of routine. He was the most human of men; wanted to know about the food the unit was getting: whether our quarters were comfortable: what recreation we had: was anyone interesting themselves in our welfare: and much else. He got his first car during this year, and its delivery put an end to his taking a taxi to the hospital which was on the outskirts of Glasgow, several miles from his house or from the University department. In the days of the taxi, he dismissed it when he got to the hospital as he could count on being given a lift home by one of the visiting clinicians. I can vouch for it that on more than one occasion he drove out to the hospital in his car, parked it in front of the administrative buildings, spent several hours at the laboratory and then accepted a lift home in a colleague's car. It was at this time, too, that there occurred the episode of the ward orderly springing to attention and clicking his heels smartly as Bobby, in his new uniform (he had previously been in civvies only), passed along the corridor and doubtless far away in thought. The sound of the clicked heels. and the salute given, startled him; and, staggering a little sideways, he said "D . . . d . . . d . , . don't do that." It was something of a problem to keep him supplied with matches for lighting his many cigarettes, or his pipe. We had no gas at all, and were dependent on spirit lamps for all purposes for which a Bunsen burner is ordinarily used. Bobby never noticed a burning spirit lamp (always on the bench for his convenience) and so repeatedly asked "Have you a match?" The proffered box was used and then dropped into his pocket; and ere long "Have you a match?" was heard again. One day I had a stock of matches laid in prior to what was going to be a lengthier session than usual and, whenever I saw him get out a cigarette, I laid a box well in his field of view. One by one the boxes disappeared into his pocket, and he went off ultimately with nine of them. He was up again next day, and first thing when he came in he fished eight boxes of matches from his pockets and arranged them in a little tower on a bench. He said nothing, but gave me one of his whimsical smiles! He was invariably courteous to all comers, even for such as were "difficult" and for whom he had pretty clearly not much regard. I never saw him show even a shadow of annoyance; on only one occasion, after a post mortem at which the clinician had been boorish, was he moved to remark to me "That's a dreadful man."

'I became an assistant in the Pathology Department of the University in 1919 and, as it turned out, I stayed with him for thirteen years. To me he was unique, and ever will be—as teacher, as "Chief", and as friend. He was the easiest of guests to have staying with one; he made himself completely at home and radiated kindliness quite spontaneously. He was utterly devoid of affectation—he had no time for it in others—and was genuinely interested in others. He was very fond of golf, and sought to encourage all his staff to take it up. He kept at me repeatedly about this; and when I told him about cycling, walking, hill climbing, and what not, he said "Oh! yes, these are all right, but there's something special about golf", and then—after a lengthy pause—"You know, golf isn't just a game; its a psychological and neuromuscular exercise!" He bore me no grudge, I dare to believe, but I must have been a disappointment to him. On another occasion, but in wholly different circumstances, I overheard a remark of his that is worth relating again. At a dinner given by members of his staff, past and present, to mark his semi-jubilee in the Glasgow Chair, the man on his right was a teetotaller and had been imbibing lemonade throughout the evening. Bobby turned to this excellent man (whom I will call "X") towards the close of the reunion and said, "X, do you always drink that stuff?" X said that he often took it as he liked it. Then Bobby sketched a verbal picture of a round of golf on a blazing summer afternoon, and of the satisfaction of getting back ultimately to the club-house, and said "X, what would you have on such an occasion?" X thought for a bit, and replied "I think I would have a glass of milk". And to this Bobby's instant rejoinder, uttered with the deepest feeling, was "Not *NEAT*, X, not *NEAT*?" And here's another memory: I had been spending a week-end in the home of a man who had earlier been on the staff of the Glasgow department and whom Bobby liked very much, and whom I'll call "A". One day after my return, and while we were having tea in the little library of the department, Bobby inquired how A's young son was getting on. I replied that the youngster was flourishing, and was getting crushed carrots and strained tomato juice as an essential part of his diet. Bobby said "Good Lord", and lapsed into silence. A little later he looked across the table to me and said "*What* did you say the youngster was eating?" I replied "Crushed carrots and strained tomato juice". "Good Lord", ejaculated Bobby, sat in silence for a few minutes, and then went off to his room. Not long afterwards the door of my room was opened and Bobby came in. "*What* did you say that youngster was having?" he asked. "Crushed carrots and strained tomato juice", I said once more. "Good Lord", said Bobby, "were you fed on crushed carrots and strained tomato juice?" I said that I didn't think so and then, in a voice charged with deep feeling (apparently) he said "Do you think that *I* was fed on crushed carrots and strained tomato juice? Good Lord", and away he went. He was obviously shaken by this introduction to modern trends in infant feeding!'

An assistant who joined him in 1924 recalls his initiation into research. 'While I was doing some odd jobs in accordance with one directive, he would sit silent by the window with his back to me, looking out over the roof-tops of Church Street for 15 minutes or longer. When I ventured to break the silence he often said "S-sh, I am thinking!" and then, after another long silence, he would issue another directive which seemed to me to bear no relationship whatsoever to the previous one or the one before that. In such an emergency, when a session of two hours perhaps was over, I would go along to Carl

Browning and he, with his greater experience and better understanding, kindly helped me to close the gaps. As I learned several years afterwards, the results I persisted in getting were diametrically opposed to his expectations, although he eventually accepted them. From that point of view, he was not an easy chief for a beginner like me to work for, possibly because, being the gentleman he was, he did not wish to hurt my feelings. Later on, having given me a job to do, he left me entirely free to do it in my own time without any fuss, however keen he might be to hear the result. By keeping me guessing, so to speak, I suppose he did encourage me to think for myself, but all the ideas and the planning of experiments were entirely his own and, more often than not, I was his bewildered agent. Nevertheless, it was my pride and joy to work for him, often to midnight or later in the laboratory, and the merest hint of his approach thrilled me.

'As golfers, he and I were of different species, he a tiger in his day and I a rabbit. It wasn't until I joined his staff at the mature age of 30 I learned from him that proficiency in golf was essential to progress in pathology! In the 1920's and early '30s, he used to go to Newcastle, Co. Down, with three other friendly tigers for a fortnight at Easter to play two rounds a day in all weathers—their aim in every round being to return a best-ball score of less than the average of their ages, i.e. about 68 or less. During 1932, he commanded me to join his party with my golf clubs. Being the perfect host he was, he gave me the honour at the first tee and, praying to all my gods, I hit a pretty long ball (for me) *nearly* straight down the fairway—but not quite, as it drifted a little bit to the right over a distant and harmless-looking dune. Knowing nothing of the course, I was about to render silent thanks to my gods when he remarked "You'll never see that one again". Surely enough it was lost amongst countless white pebbles on the beach of the Irish Sea. He bore the rest of the match with Christian fortitude and then, telling me that he couldn't play the game himself but knew at least how to teach it, he took me to the practice ground behind the club house, altered my grip, my stance, my everything and put me through my paces for a solid hour.'

A distinguished assistant who knew Muir in his later period gives a vivid account of his way with research students. 'In my final honours year the only formal teaching I got from him covered a period of less than ten minutes! He came into my cubicle, borrowed a bit of paper and a pencil (probably accepted my matches for keeps), made a little, very rough diagram of the skin and scrabbled a bit in the epithelium and then again in the dermis and said Masson thought melanomata arose from cells here and cells there and that I'd better read it up. The other thing was to hand me some blank slides (from G. B. Shaw) and ask me to stain them: they were from a case of rheumatic carditis of a florid type and when I thought they were rather more exciting than the textbook story he suggested that I'd better try and find some more. Somehow out of simple remarks like that, people like me went off and scrounged rheumatic material from the district and went into the whole technique of getting thin sections of heart and wrote a thesis on rheumatic morbid anatomy. Other people did likewise on the stimulus of no more than one of his simple unemotional statements. Many of us in Glasgow used to ponder how R.M. did it. We could find no rational explanation. He worked away quietly himself—not obviously very keen on interruption, although a surgical specimen was always a complete excuse and everything else was put aside if an opinion was wanted on a biopsy slide. I don't think he as much as said so, but distinguished visitors, experimental work, practically everything took second place to a biopsy problem—except perhaps the 2 o'clock class. We never interrupted him before it and he was punctual to a degree, crossing the corridor one second ahead of the clock.

'The shyness of earlier days made him very unhappy at oral examinations and his combination of distress about the candidate and difficulty in producing the necessary child-talk for the dumb and dim, gained him the quite fallacious reputation among students of being befuddled. R.M. became much less shy in later years. He never bragged and was pretty near to being secretive about his family history. I don't think many men found him an easy chatty conversationalist, even his friends and those who admired him for his sincerity. On the other hand, he wasn't very thrilled with dull people. Another mark of the Celt was his gift of being the same to all men; he treated the technicians with the same attitude as he did the most grandiloquent clinician or the stuttering junior.'

Finally, some recollections of the years of retirement. 'His early years in Edinburgh after retirement were full of interest for him. With his old friends Ralph Stockman and Stuart MacDonald he attended the botany demonstrations given weekly by their friend Sir William Wright Smith, who died about a year before Bobby, also at a great age and in office. R.M. founded a dining club, known locally as the Knights of the Round Table, which dined every third Tuesday in the New Club, and included Wright Smith, Sydney Smith, James Learmonth, Henry Wade, Andrew Davidson, James Graham and Archibald Sinclair. Bobby chose the menu and did the accounting.

'So far as I am aware, his last fishing was in Islay with D. F. Cappell in the summer of 1957 when he was the only one of the party to kill a fish—in Scotland, that means a salmon. Of course, he was then very frail and he may have done no more than hold the rod but the fish was his. He had given up golf a few years before though he attended the famous annual golf match between the Senates of Aberdeen and Glasgow Universities. Latterly he had a lot of time on his hands and did not go out in the evenings, but his many friends saw to it that he had motor runs and brought him to University functions and Royal Society of Edinburgh lectures. He entered into the cultural life of Edinburgh and maintained his interests until within a few weeks of his death, when the weather kept him from the Club. He was loved there and at his death some of the waitresses wept most of the day.'

Let his life-long friend Professor C. H. Browning, F.R.S., say the last word. 'In the performance of duty and exercise of authority he showed rare wisdom and tolerance and was most magnanimous in attributing motives. In his daily life he succeeded in cultivating the whole man.' The rest of us can do no more than echo Xenophon's farewell to Socrates: 'And so, in contemplating the man's wisdom and nobility of character, I find it beyond my power to forget him or, in remembering him, to refrain from praising him.'

Scientific contributions

Muir's scientific investigations fall into four clear-cut groups. His earliest studies, stimulated no doubt by W. S. Greenfield's justly celebrated knowledge of the blood, faced the problem of where and in what manner are the blood cells formed and how do upsets in their development bring about disease? This interest led on to the studies in immunity since at this time the humoral school of Ehrlich had clashed vigorously with the cellular protagonists, headed by Metschnikoff, in a battle royal over the defensive properties of the blood. Muir, like many young researchers with mathematical leanings, was attracted by the rigorous quantitative methods of Erhlich rather than the more speculative, intensely teleological outlook of Metschnikoff. Since techniques based upon the destruction of red corpuscles play a big part in immunological research, it is not surprising that Muir became more and more engrossed in haemolysis and extended his investigations on that phenomenon to the living animal. Haemoglobin is one of the great iron reservoirs in the body and its disintegration leads to redistribution of iron in the tissues, some of which possess cells that are largely concerned in the storage and metabolism of the element. These cells, now grouped together usefully as the Reticulo-endothelial system, are closely related to the blood cells so that Muir was back again on familiar territory. His interest in this field of investigation brought him the friendship of the great German pathologist, Ludwig Aschoff, who had been largely responsible for the doctrine of the Reticulo-endothelial system. Muir's last work centred around the malignant cell, which he approached through certain challenging problems associated with cancer of the breast. As one whose opinion on every kind of pathological tissue was widely sought, Muir must have known the despair and frustration that comes to all conscientious pathologists when confronted with malignancy. Cancer of the breast is among the most poignant of these demands since it is so often encountered in active women with families who conceal it until too late because of personal modesty. Many pathologists become more and more engrossed in the tumour problem as they grow older, perhaps because of a subconscious desire to add their modicum of experience to the all too scanty fund that accumulates so slowly over the years. A kind of despair seems to grip them in their decline as they reckon up the disappointing returns of their life-long efforts to fathom the mystery. No-one can have been more clear-minded about these issues than Muir; perhaps he won a grain of comfort from his elegant investigation into the evolution of cancer of the breast and the way it might lead to Paget's disease of the nipple.

Blood cells and their diseases

After introducing various simple improvements in the technique of studying blood and bone marrow cells, Muir sets about the search for relationships between the two types of cell. His early studies of normal material do not get him very much further so he turns to two great varieties of blood disease, leucocythaemia which we now prefer to call leukaemia, and pernicious anaemia. The former, he decides, is 'an excessive and indefinite proliferation of a certain kind of cell in its nature resembling that seen in the growth of many tumours.' It is the white cell family that is affected and the blood picture reflects a pathological condition of the bone marrow in which there is 'an extension of the bounds of the bone marrow so that its cells appear in the blood.' Although two types of white cell are proliferating abnormally they actually spring from a single stem cell component of the marrow. In pernicious anaemia red corpuscle development is disturbed and many immature nucleated red cells, some of which are very large, replace the yellow marrow in the shafts of long bones. This new red marrow closely resembles normal marrow in many ways and may well be a kind of compensation for blood destruction since it is very like that which is formed when animals lose blood. In this view, Muir and many others were only partly correct but it is difficult to see how they could have escaped from their conclusion.

These early papers are full of good, sound observations and are models of morphological research. A short communication on purpura and intense anaemia draws attention to the extensive deposits of iron-containing pigment in various organs together with widespread fatty degeneration of cells where haemorrhage, the most outstanding feature of the disease, has occurred. Muir also comments on the iron deposits in organs from sufferers from pernicious anaemia and these significant observations may well have formed the basis of his life-long interest in red cell destruction and its associated disturbance of iron metabolism. But the theme to which he returns time and time again is the production of the white corpuscle. In one of his most elegant investigations carried out in 1901 he turns to the experimental method for help. Since one ounce of pus may contain, he calculates, as many leucocytes as 120 ounces of blood, and many ounces of pus may be formed in a few days of infection, a remarkable supply of leucocytes must be available in the body for emergencies. Why not attempt the reproduction of this phenomenon in animals and see if the flood of leucocytes can be traced from the bone marrow, the most likely source, to the tissues by way of the circulating blood? Rabbits were injected with not-too-virulent strains of pus-producing bacteria (especially *Staphylococcus aureus*) and the bone marrow and blood examined at various stages thereafter as the infected tissues became inflamed. Muir shows that the number of white corpuscles in the blood at first declines rapidly because so many cells leave the blood vessels at the site of infection to become pus. Then their number increases very considerably, the granular leucocytes responding first, the hyaline (mononuclear) cells much later, the eosinophilic cells not at all. Immature red and white corpuscles enter the blood when the infection is severe or blood poisoning supervenes. Meanwhile the bone marrow proliferates and the granular myelocytes, especially, multiply and occupy most of the marrow space. Myelocytes divide by mitosis to give rise to typical granular leucocytes, thus affording a source of renewal of those lost during the acute inflammation caused by the infection. Here again we are faced with an example of 'compensatory hyperplasia'. Presumably, bacterial products are absorbed at the site of infection and are carried in the blood to the bone marrow where they stimulate the germinal cells to divide. Leucocytosis is an indication that marrow can replace the phagocytes that are lost during the defence of the tissues against infection. Sometimes the chemotactic agents may prove to be poisons for in severe infections blood and marrow cells, including red corpuscles, are seriously damaged or killed. These are quickly removed from the blood by the spleen and, to a lesser extent, the lymph glands. During disposal the red cells are lysed and their iron is stored within the splenic cells, there to be used again as the need arises.

Muir's early researches on blood cells mark him out as an investigator of considerable power. His demonstration that the leucocytosis of certain infections comes from the proliferation and differentiation of the granular myelocytes in the bone marrow is masterly and highly provocative.

Studies on immunity

In the 1890's, the science of immunity made a rapid step forward when antitoxins, first for tetanus, a little later for diphtheria, were demonstrated in the blood. These were shown to combine with their corresponding toxin so as to form something that was inert towards the cells of the body; beyond this neutralizing action they had no other effect. A great many antigens, other than toxins, including ferments, bacteria, serum, milk or cells of another species were soon found to produce anti-substances when they are injected into an animal. In some cases the anti-substance interacts with the corresponding antigen to give distinct and easily recognized changes *in vitro*, e.g. precipitins precipitate the protein of that serum for which they are specific, bacteria induce agglutinins that clump or agglutinate them.

An important advance came when Pfeiffer showed that the serum of animals immunized against typhoid or cholera germs develops the power of killing them because of the interaction of two distinct substances:

1. immune body, amboceptor or substance sensabilisatrice, an anti-substance specially developed during immunization;
2. complement or alexine present in normal serum.

Two corresponding substances are concerned in all cases where a dissolving or lytic action is developed during immunization. Thus red corpuscles interact with haemolysin in the presence of complement to undergo disruption (haemolysis). The immune body anchors itself both to the corpuscles and to complement. Hence corpuscles sensitized with the corresponding anti-serum heated to destroy any complement that may be present serve as a test for the presence of complement, for if complement is now offered to the system the corpuscles lyse; if it is absent or used up in any way, no lysis occurs.

Each anti-substance is specific within certain limits, i.e. each anti-substance exerts its effect only against the antigen that has led to its production. Anti-substances are formed not only in response to an artificial inoculation, but during the course of an attack of the natural disease. Hence a disease may be diagnosed by demonstrating appropriate anti-substance in the serum of the patient, especially agglutinins and immune bodies. The latter join with the corresponding bacterium to fix or deviate complement and thus interfere with the lysis of sensitized corpuscles when these are added to the mixture. Complement deviation—the Bordet-Gengou phenomenon—can be used with a great many antigens. A special variant is the Wassermann reaction for syphilis which is highly specific for the disease despite the proof by Marie and Levaditi that extracts of normal liver and other organs can be substituted for syphilitic antigen, and the even more remarkable demonstration that many lipoids are just as effective as antigen. The explanation seems to be that syphilitic serum contains a substance (probably a modified protein) that fixes complement in the presence of lipoids.

Other anti-substances have been found which are more or less effective against the appropriate antigen. They include opsonins which make bacteria more vulnerable to the digestive action of phagocytes. A simple explanation of anti-substance production and action was given by Ehrlich in his famous side-chain theory according to which receptor groups or side-chains of cells link with immune body and complement, or directly with anti-toxin, precipitin or agglutinin. During immunization receptors are produced by the cells in excess and are discharged into the body fluids to act as free anti-substances.

Such was the general theory of immunity and, indeed, it still holds with certain modifications, when Muir and his colleagues C. H. Browning, F.R.S., A. R. Ferguson, W. B. M. Martin and J. W. McNee (now Sir John McNee) made their contributions. Their work is fully described in Muir's *Studies on immunity* published in 1909 and in his contributions to the 6th volume of the Medical Research Council's *System of bacteriology* that appeared in 1931. The most significant contributions will be described in terms of the three essential components of the antigen-antibody reaction.

Cell receptors or side-chains

The receptors of red corpuscles that are concerned in haemolysis are comparatively stable molecules. They are not destroyed when the corpuscles are laked by water or ether and the haemolysate can take up as much complement through the medium of immune body as the original suspension had. The interaction of receptor, immune body and complement goes on as readily, after disintegration, as when the corpuscles are still intact. However, most of the receptors remain attached to the corpuscular stromata and will sediment with them although some can be detached and go into solution in the suspending fluid. They behave differently according to the nature of the agent used for cell destruction, water being a more powerful disrupting force than a specific haemolytic serum. Receptors are very stable when exposed to high temperatures, many resisting exposure to 100 °C for 1 hour.

Immune body

Immune body does something more than bring complement into combination with receptors. Thus horse complement in the presence of rabbit immune body and ox red corpuscles gives no haemolysis although a considerable amount of complement is joined up with both, as shown by complement deviation tests. On the other hand, horse complement with cat immune body and ox corpuscles gives lysis. The two immune bodies thus appear to combine with the same molecules of the red corpuscles, yet in one case complement lyses the cells, in the other it combines yet does not lyse.

Immune bodies from one species may prevent the combination of immune body from another species. The two immune bodies in such a case apparently unite with the same receptors but each excludes the other. Hence immune bodies possess specificity of two kinds:

1. they are specific in relation to the antigens used for their production;
2. they are specific in respect of the animal from which they have been derived.

The results in a haemolytic experiment depend chiefly on the complement and the corpuscles used. But it is not immaterial, for the amount of lysis, by which immune body the complement is brought into combination with the corpuscles. More efficient lysis may result with one immune body than another, the same complement being used in the two cases, and this even when it can be shown that the immune bodies unite with the same receptors of the corpuscles. These facts are best explained on the assumption that the immune body, although not producing lysis by itself, acts as an auxiliary toxic agent in addition to leading to the union of complement.

The combination of immune body with red corpuscles is a reversible phenomenon as was demonstrated independently by Morgenroth and by Muir in 1903. The rate of dissociation depends on the temperature at which the reaction is maintained, scarcely any occurring at 0 °C. Dissociation also occurs within the animal body when sensitized corpuscles are injected intravenously. When a rabbit is injected with its own corpuscles previously sensitized with immune body, the lysis that follows is not confined to the sensitized corpuscles but also occurs in the blood generally. This can only mean that immune body is dissociated from the injected corpuscles by some agency to a greater degree than occurs by simple physical dissociation. Even more strange is the fact that when large amounts of complement are fixed to the receptors of red corpuscles through the medium of immune body the latter can be dissociated from the corpuscles while complement remains fixed. This does not support Ehrlich's view that complement combines directly with immune body. Hence the direct union of complement and immune body is not proven. The same criticism applies to any antigen-antibody reaction.

Immune body itself possesses antigenic properties and an anti-immune body can be developed against it.

Complement

The union of complement with sensitized cells is most likely physical since it can be prevented by the action of hypertonic solutions and by protein. Haemolysis occurs less rapidly when the corpuscles are suspended in blood-serum and a much larger dose of complement is required than when saline is used as the medium. Normal serum when filtered through a Berkefeld filter loses complement; hypertonic suspensions of complement pass through unchanged and the complement thereafter can be removed by dilution. Complement thus can be defined as that labile substance of normal serum which is taken up by the combination of an antigen and its anti-substance or immune body. It may be haemolytic, bactericidal or bacteriolytic, indeed, it may also be opsonic or agglutinitic. It differs from ferments in being used up and fixed to cells depending on the presence of immune body.

It is quite unlikely that complement of serum is made up of similar molecules. Variations both in the combining affinities and in toxic effects are met with, while differences in physical properties are well known. Muir discovered a remarkable case in which a part of the complement acted on sensitized guinea-pig corpuscles but not on sensitized ox corpuscles. More striking still is the absorption of complement by bacteria, for the bactericidal component of such complement can be taken up by a great many different bacteria before the haemolytic moiety is appreciably affected. We seem to be concerned here with a kind of preferential absorption, the molecules with greatest affinity combining first. Differences in degree of combining affinity are therefore significant and may explain things simpler than by postulating special combining affinities for complement as Ehrlich suggested. Complements of different animals differ widely, too, in their relative toxic action.

The opsonic property of serum, like the bacteriotropic property, is practically abolished by powerful complement absorbers. The thermolabile opsonin of normal serum belongs to the group of complement, but the thermostable opsonin of anti-serum is a true anti-substance with the comparatively specific characters of anti-substances in general. Probably it has the constitution of an agglutinin.

Complement deviation and the Wassermann reaction

McIntosh and Browning, followed closely by Mackenzie and Fildes, were the first to use these tests in Great Britain, not only for the investigation of the underlying principles of immunity but also in the practical application of the reaction to diagnosis. Without doubt the analytical work planned by Muir on antibody-complement mechanism and summarized in the preceding paragraphs prepared the ground for these fruitful advances.

The nature of immunity

In Muir's opinion the programme of immunity cannot be considered as synonymous with research into the serological changes in the immune animal. In 1931 he wrote: 'It is known that important changes occur in the cells of the body as the result of the functional activity in producing antibodies or apart from such function.' The evidence for this conclusion is that

1. 'it is known that after antibody response has occurred the functional capacity of the cells, so far as antibody production is concerned, has undergone alteration, and that immunity may persist after antibodies have disappeared from the blood.'
2. 'in the state of anaphylaxis which also depends upon antibody production, the tissues of the sensitized animal respond to antigen in a different way from those of a normal animal.'
3. 'local immunity may be produced by bacterial vaccines without there being any evidence of antibodies being concerned.'

Are we very much further on in our ideas today?

Blood destruction in the tissues of the body

His immunological investigations no doubt aroused Muir's interest in the effects of blood destruction since animals injected with red corpuscles sensitized with immune body destroy these cells in a short time with the liberation of their haemoglobin and its excretion in the urine. This induces anaemia which, however, is not a dilution effect for the blood volume is not increased. As a result of this anaemia the red bone marrow manufactures new red corpuscles so rapidly that some of the new, immature cells overflow into the circulation where they are easily recognized by their morphological characteristics. This latter process is a true compensatory mechanism designed to replace the lost haemoglobin and maintain a proper oxygen supply to the tissues. Once regeneration starts it goes on very rapidly and a rabbit may manufacture as many as half a million red corpuscles per cubic millimetre of blood each day until recovery is complete. What happens to the haemoglobin liberated when the red cells are destroyed? Its fate is decided in certain organs which have learned the trick of conserving free iron by abstracting all but minute amounts of these compounds from the blood. A rabbit weighing 1500 g stores about 7 mg of iron in its blood-free abdominal organs as against 0·45 mg of iron per ml. of its blood. When more than half of its blood cells is destroyed by a haemolytic serum the mean percentage of iron in the liver and kidneys increases about five times, that of the spleen threefold. Since relatively little haemoglobin reaches the urine and none escapes by way of the bile unless the liver cells have been damaged, this iron increase suggests that the free haemoglobin is abstracted by those organs and its iron stored in their cells. But the position is not so straightforward as that, for large amounts of haemoglobin can be injected rapidly into the circulation of rabbits without any effect other than a rather mild increase in iron in the kidneys. The liver certainly shows no sign that it has taken up haemoglobin and formed simpler iron compounds, and the spleen remains unchanged. Even when haemoglobin is introduced repeatedly into the blood stream the iron piles up in the liver very slowly and does not reach the level met with when the corpuscles are destroyed by a haemolytic serum. Clearly these experiments give no support to the view that haemoglobin as such is taken up by the cells of the liver and broken up there. The position still remains much as it was when Muir abandoned it in 1935.

For many years the great German pathologists were quite sure that haemoglobin let loose in the tissues is converted into haemosiderin, which is the iron-containing part of the molecule, by intracellular action and haematoidin, the iron-free portion almost certainly identical with a bile pigment, which is formed extracellularly. Any haematoidin deposits within cells were thought to be the result of phagocytosis of crystals formed outside of these cells. In 1924 the American pathologist, Rich, showed that both compounds are formed intracellularly when red corpuscles are added to tissue cultures of mesodermal cells. Muir and Janet Niven introduced red corpuscles into the subcutaneous tissues of rats and mice and watched their ingestion by the local phagocytes. Diffuse and granular forms of haemosiderin were found within these cells after 24 hours but haematoidin needed 7 days for completion. The latter gave the Gmelin reaction for bile pigment. Haemoglobin goes through similar changes. Strange to say, rabbits produce no haematoidin under these circumstances. Such changes closely parallel those seen in the brain after a cerebral haemorrhage in man. The mechanisms concerned are wholly cellular, and differ quantitatively from those which control the production of bile pigment by reticulo-endothelial cells. Iron deposition in the liver and spleen after haemolysis thus may be the consequence of functional activity on the part of the phagocytes that form a portion of these organs. Similar principles apply in the organs of human beings suffering from pernicious anaemia and bronzed diabetes (haemochromatosis). The liver in the latter condition may contain more than one hundred times its normal iron content, but there is no evidence of haemolysis or compensatory marrow increase. In pernicious anaemia, however, the red marrow is greatly increased in amount and haemolysis is quite likely an important factor.

Cancer and the malignant state

Muir refrained from writing on cancer until he was in his 60's, yet all of his life he had, as a hospital pathologist, been appalled by the ravages of that disease. From 1927 to 1941 he recorded his experience with cancer of the breast in a series of masterly papers that explain the puzzling relationship long known to exist between an eczematous condition of the nipple, labelled Paget's disease, tumours of the milk ducts and the more common varieties of cancer of the main breast substance. In 1927 he developed the theory of Jacobaeus that Paget's disease results when cancer cells arising within the ducts of the breast grow into the epithelial covering of the nipple. Muir proved that the peculiar, swollen, vacuolated cells long held to be the hallmark of Paget's disease are really tumour cells of glandular type which reach the nipple epidermis from the upper extremities of malignant lactiferous ducts. Such intra-epidermal spread induces reactive changes in the underlying connective tissue of the dermis with congestion of the associated blood vessels and liberation of a weeping exudate that is responsible for the eczematous transformation of the nipple. More severe spread may bring about destruction of the invaded epidermis and ulceration of the skin over the breast.

The emphasis placed upon the initiating intra-duct cancer is of great practical importance, for at this stage it is lowly malignant and insidious so that it may be overlooked for a long time. If unchecked, the malignant transformation spreads along the branches of the main ducts into the depths of the organ and sooner or later the cancer cells break through the internal lining of the ducts and grow unchecked within the interstices of the breast. The consequences of this unrestricted infiltration are catastrophic for they culminate in the distant spread of the cancer to various internal organs of the body and this leads to death. Both Paget's disease of the nipple and ordinary cancer of the breast thus are possible sequela of the same disease—intraduct cancer. However, Paget's disease occurs only when the intraduct carcinoma is present in the upper reaches of the nipple ducts, a relatively uncommon occurrence, so that in actual practice it is a rare occurrence. Much more frequent is the spread of intraduct cancer deep within the breast and its break through the duct lining to produce a rapidly growing tumour that destroys the breast tissue.

Nevertheless, the position is not so gloomy as it sounds for two very good reasons. In the first place, malignancy is often confined to the intraduct and intra-acinar passages for a long period, during which there is a reasonable chance of successful treatment, especially when diagnosis is established as early as possible. And secondly, the cancer cells sometimes undergo spontaneous retrogression and may even disappear completely from some or many ducts. As they disappear the fibrous tissue around them thickens and becomes tough and glassy, the mantle of elastic tissue, so characteristic of normal mammary ducts, increases in amount and associated blood vessels disappear. These changes which affect more often the ducts in the deeper part of the nipple and the subjacent tissue than the substance of the breast may bring about further elimination of intraduct cancer cells, but destruction is never complete. Still, the outlook in such cases is encouraging and energetic treatment is much more likely to succeed here than with the ordinary type of cancer.

In Muir's last contribution to this subject he again emphasizes the above sequences and describes the relationship that exists between duct papillomas and cancer. He takes great pains to stress the multicentric origin of cancer no matter whether the latter arises in association with papilloma, cystic hyperplasia or intra-acinously. He concludes that the endless variety of cell configurations and arrangements in each of these conditions reflects the abnormal action of hormones or substances allied to organizers rather than the reaction to a definite irritant such as a virus. He sees no evidence of the dual action of diffusely-acting hormone and a particulate agent leading to focal malignancy as a separate process.

It is fitting that this account should end with a few words about Muir's final summing up on the malignant state 'after a relatively long life as a pathologist'. In a general way malignancy may be regarded as an abnormal behaviour of cells, consisting essentially in uncontrolled growth 'shown by deviations in cell structure, de-differentiation or anaplasia and sometimes extreme aberration in type and it is often characterized by metabolic changes especially in respiratory function'. The change is permanent for malignant cells do not become normal again and the change is transmitted to their descendants. One general principle is peculiar to the initiation of malignancy —it is related to previous non-neoplastic proliferation which may be intermittent or delayed. It may supervene on cell proliferation brought about in various ways by irritants, cell stimuli and hormones and as a result of compensatory hyperplasia. In most cases a long interval elapses between the onset of proliferation and malignancy. With chemical irritation the tendency to initiate malignant change is in certain instances related to molecular structure. Inherited susceptibility plays an important part, especially in certain organs, and in some cases congenital abnormalities may be important forerunners. All of these factors may be combined in different ways.

These were his last well-considered thoughts on a matter he considered of paramount importance and one that had baffled him, as it has baffled all great pathologists, at every stage of his intellectual development. Nevertheless, he felt certain that some day an answer would be found through the medium of experimental cytology and he was well content that this should be so.

Other contributions

Muir's gifts as an investigator are well shown in an early paper on pseudotuberculosis (1898). One of his medical students brought along a small singing bird whose liver and spleen were studded with small yellowish nodules. From these he obtained a bacillus which turned out to be closely related to *B. pseudo-tuberculosis* of Nocard. He produced the disease in other birds and in rodents and gave a full description of its pathology and bacteriology, noting its resemblance to glanders. With Haswell Wilson he investigated thoroughly the lesions of influenza that swept the world in 1918-1919. His observations on regeneration of the liver and on a unique case of a liver cancer in a child of nine, which he considered congenital in origin, and on coronary arterial block are still of value. And finally, the lectures that he delivered to various medical societies, including the fourth Listerian Oration before the Canadian Medical Association, stand out as models of clarity and learning. Of his text books, many generations of students and teachers have shown their approval both at home and abroad. The *Manual of bacteriology*, first published jointly with James Ritchie in 1897, has reached its eleventh edition. The *Textbook of pathology* achieved five editions between 1924 and 1941 and continues its successful course under the devoted guidance of his pupil D. F. Cappell.

The writer of this memoir acknowledges the generous assistance he has received from many of Sir Robert Muir's distinguished assistants, especially D. F. Cappell, E. M. Dunlop, A. S. Lendrum, J. S. Young, J. W. McNee, G. L. Montgomery, C. H. Browning, J. W. S. Blacklock; from Miss C. T. Gilchrist, Secretary of the Lady Artists' Club, Glasgow, the Secretary of the Royal Scottish Society of Painters in Water-Colours, the Director (in charge of Art) of the Glasgow Art Gallery and Museum and the Librarian of the Central Medical Library, University of Edinburgh.

The pencil sketch, reproduced with the photograph as a frontispiece, is the work of Sir Robert Muir's sister, Anne.

ROY CAMERON

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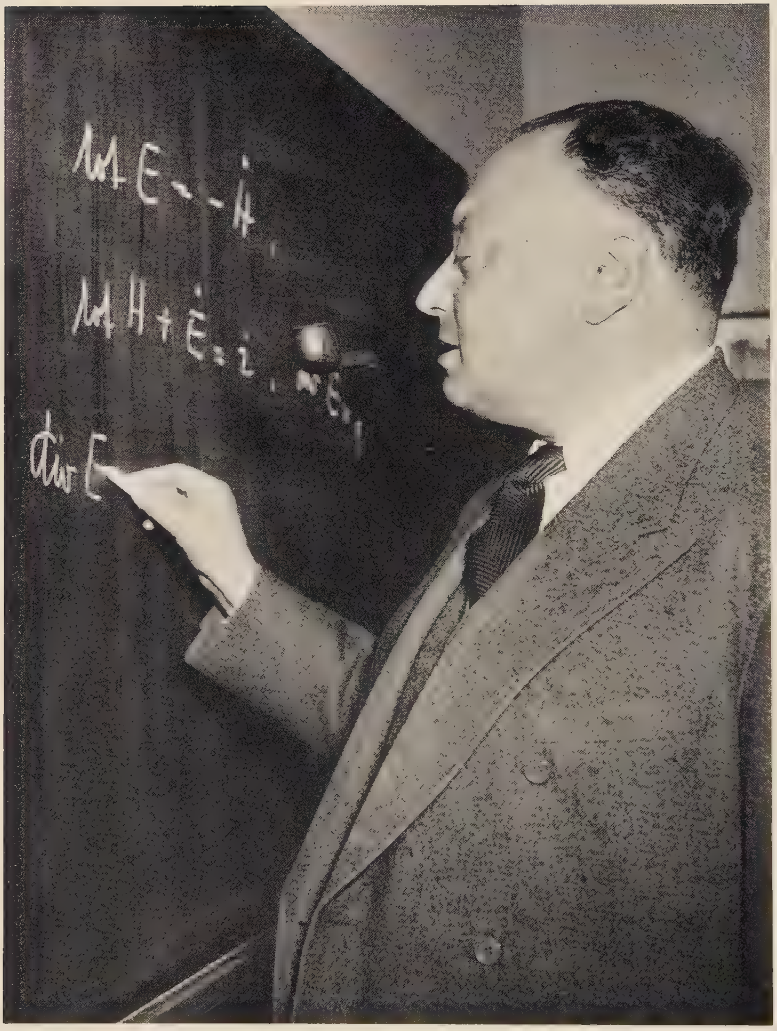
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W. Pauli

WOLFGANG ERNST PAULI

1900-1958

WOLFGANG PAULI was born on 25 April 1900 in Vienna. His father, Wolfgang Joseph Pauli, was distinguished as a biochemist and was a Professor in the University of Vienna; previously he had practised as a doctor, and his patients included many prominent figures in Vienna society. The mother, Bertha, *née* Schütz, was a writer and had many contacts in the world of the theatre and the press. It is probable that this background and the acquaintance with the leading authorities in many fields had a profound effect in creating the high standards and the impatience with anything but the best of its kind, which became later an important characteristic of the young Pauli.

The young Wolfgang showed early signs of exceptional ability. He was outstanding at school in scientific subjects and particularly in mathematics, and first-rate in all other subjects, except in languages in which his performance was good but not exceptional. He was probably a 'bookish' child and was not interested in games, though he was, and remained throughout his life, fond of walking, particularly in the mountains, and of swimming. As a small child he disliked fairy tales, which seemed to worry him. As a growing boy he was fascinated by books like those of Jules Verne, and he later developed a special interest in astronomy. His younger sister, Hertha (now Mrs Ashton), had to serve as an audience for his current discoveries in astronomy, and he was most impatient when the audience did not respond adequately to the instruction.

In 1918 he became a student in the University of Munich under A. Sommerfeld, and he obtained his Ph.D. there after three years, the shortest period allowed by the University regulations, and exceptionally short for a subject like theoretical physics.

Sommerfeld's group in Munich provided a stimulating environment in which Pauli found it easy to develop an understanding of the whole of current theoretical physics and of the mathematical methods required for its study. Sommerfeld was one of the best teachers, with his physical intuition and his command of mathematical tools; he was particularly successful in getting his pupils to work on problems which challenged their ability and initiative, yet were certain to lead to concrete and quantitative answers. At the time he had a collection of brilliant young men working with him, but he recognized at once the exceptional promise and maturity of the young Pauli, and he arranged for Pauli to be entrusted with the writing of the article on relativity for the *Encyclopaedia of Mathematical Sciences.*

As a result Pauli completed during the three years at Munich not merely his Ph.D. dissertation on the hydrogen molecule ion (9)*, probably the most ambitious application of the old Bohr-Sommerfeld quantum theory that has ever been attempted, but also his article on relativity (94), which has remained to this day unrivalled as a survey of this field. An English translation with a brief review by Pauli of the developments since 1921 has recently been published.

Pauli always remained conscious of how much he owed to Sommerfeld. In later years it was surprising, when Sommerfeld visited him, to watch the respect and awe in his attitude to his former teacher, particularly striking in a man who was not normally inclined to be diffident. He would himself laughingly refer to his 'Schüler complex'.

Working on his Ph.D. thesis in the daytime and on his relativity article at night did not leave him much time for the more mundane problems of life. He held a small grant from the University, but his father, who was anxious that the son should not be in need, sent some money to supplement the award. However, the early nineteen-twenties saw the beginning of the German inflation and, by the time young Pauli remembered to go and claim the money from the bank, it had become practically worthless.

He spent the academic year 1921-22 in the University of Göttingen, another centre of modern theoretical physics, and the following year in Niels Bohr's Institute in Copenhagen. This was the beginning of a very close and warm friendship with Bohr, renewed in many later visits.

In 1923 he was appointed to a post in the University of Hamburg where he became Privatdozent, and he remained there until 1928. He always spoke of his period in Hamburg with particular pleasure. It must have been a period when he began to sense his power and to realize that his work in physics came up to his own high standards. At the same time, largely under the influence of Otto Stern, he began to take more interest in practical matters and to organize his way of life to suit his interests.

He was appointed in 1928 to the chair of physics in the Eidgenössische Technische Hochschule (Federal Institute of Technology), Zurich, where he remained, with an interruption during the war, until his death.

He spent the war years 1940-45 at the Institute for Advanced Study, Princeton, and after that remained a permanent member of the Institute, which he visited on several later occasions.

After a short first marriage, which broke up almost immediately, he married Francisca Bertram in 1934; his wife's help and understanding became an invaluable factor in keeping him at peace with the world in his later years.

Turning now to Pauli's contributions to physics, his first publications were concerned with the theory of relativity, attracted to this subject no doubt as a result of his early interest in astronomy. These papers already show the incisive clarity which was to become characteristic of all his later papers and which found its early expression in the review article on relativity. But soon his main interest changed to atomic theory and, in particular, to the quantum theory.

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* Numbers in parentheses refer to the numbered Bibliography at the end of the memoir.

At that time the ideas of Planck, Bohr and others had become firmly established; it had become clear that the old Newtonian mechanics was not adequate for dealing with problems on the atomic scale, that the rules involving the quantum constant h played an essential part in understanding atomic phenomena, and that in most cases in which these rules led to unambiguous predictions, they were confirmed by observation. However, the physics of the time was still unsatisfactory in two major ways. One was that the quantum rules of Bohr and their refinement by Sommerfeld did not form a complete logical system, and in many physical problems different ways of applying them appeared to give different answers. Important examples of such difficulties were the behaviour of atoms in external electric and magnetic fields and the many-body problem.

The other trouble was that one was still using the dynamical concepts of Newtonian physics and relied on the orbits of particles calculated from the Newtonian equations of motion, selecting amongst these possible orbits certain sets which satisfied the quantum rules. This required ideas like the 'quantum jumps' from one orbit to another under the effect of radiation, which meant that the dynamical equations were applied during the motion on a steady orbit but not to the transition from one orbit to another. It also meant that one had to select in an external field orbits oriented in certain special ways relatively to this field, however weak it might be; this created a major logical difficulty, if one considered a situation in which a field in one direction was reduced to zero and then a field in a different direction applied gradually. It was difficult to see how the orbits could suddenly change from those quantized in relation to the initial field direction into those appropriate for the new orientation.

Also, since the new rules were to be applied by first finding complete solutions of the equations of motion and then selecting the appropriate orbits, their application was difficult in problems which, because of the number of degrees of freedom or because of lack of symmetry, did not lead to soluble mathematical problems.

Thus, the problems with which the early Pauli papers on quantum theory were concerned, i.e. the effect of magnetic and electric fields (4, 6, 7, 10, 13), the two-centre problem (9), and the problem of perturbation theory (8), which expresses the solution of a complicated problem approximately in terms of the properties of a similar simplified problem, related to the vital unsolved problems of the time.

One of these problems, the Zeeman effect of complex atoms, had been analyzed to a large extent. It was realized that, for example, in alkali atoms the main features of the spectrum could be understood in terms of the orbit of the last electron, regarding the inner electrons or 'core' as forming a stable inert gas configuration, which remained unchanged except at very high excitation. But applying the known quantum rules to the motion of the last electron, one predicted only half as many states as were in fact found in a magnetic field. The first suggestion to explain this was to attribute the additional degree of freedom to the core, but this view was not easily reconciled with the idea that the core was in a definite physical state and almost unaffected by the presence of the last electron and by the field. In his discussion of this problem (17), Pauli demonstrated that the evidence favoured clearly the alternative view that the outer electron itself possessed an additional degree of freedom, which allowed it two possible states for each orbit permitted by the Bohr–Sommerfeld rules, though the physical meaning of this additional degree of freedom was not immediately clear.

This view also had important consequences for the structure of complex atoms. The guiding principle in the understanding of atomic spectra and of the structure of the periodic system had been that one could classify atomic states by assigning to each electron in a complex atom one of the orbits of the Bohr theory of the hydrogen atom, i.e. one possible orbit of one electron moving in a central field of force. This picture had implied the existence of shells in the atom, a shell being made up of all the electrons in orbits of a given principal quantum number, n. But this had made it necessary to dispose rather arbitrarily of the number of electrons in each shell. For example, the K-shell with $n=1$ allowed only one choice of the Bohr–Sommerfeld quantum numbers, yet to be in line with the behaviour of atoms, one required that two, but no more than two, electrons could be found in the K-shell.

Pauli pointed out that with the idea of a new degree of freedom, there would just be two different states of an electron in the K-shell and that the correct answer could be obtained by postulating that no two electrons could be found in equivalent orbits, i.e. in orbits of the same atom with the same set of quantum numbers, including that related to the new degree of freedom.

Similarly in the L-shell one had now to allow two different states for each of the four Bohr orbits, so that with the new principle the L-shell could contain up to 8 electrons. This fitted in with the L-shell being filled in the neon atom.

Pauli realized clearly that the new principle now known as the exclusion principle, or Pauli principle, could not be expected to be a consequence of the mechanics of the motion or of the quantum rules as previously formulated, but that one was dealing here with a drastic new restriction which had to be added to the basic postulates.

The conclusions of this paper found immediate acceptance because they belonged to the type of argument whose simplicity and agreement with observation made it immediately convincing as soon as it is pointed out.

If he had made no other contribution to physics, this one paper would have assured him an important place in the history of physics and would have justified the Nobel Physics Prize, which he was awarded in 1945.

Shortly afterwards physics made a major advance through the new mechanics, first in the form of the matrix mechanics of Heisenberg and others, and almost simultaneously the wave equation of Schrödinger.

Pauli was immediately attracted by the possibilities of using these new ideas to put quantum theory on a more consistent footing. He succeeded in solving the hydrogen problem in terms of the new mechanics (20), which was a considerable achievement. The Heisenberg formulation of quantum mechanics is exceedingly easy to apply to problems with linear equations of motion, such as that of the harmonic oscillator, but the presence of the inverse square law in the attractive force in the hydrogen problem leads to mathematical problems of considerable complexity. This paper therefore added important support to the new mechanics. In his discussion of the hydrogen atom, he of course included the new degree of freedom to which he had previously drawn attention and referred to the new hypothesis of Uhlenbeck and Goudsmit, which identified this degree of freedom with an internal rotation or 'spin' of the electron, a hypothesis which by this time after some initial controversy was beginning to find acceptance.

Meanwhile, Fermi had investigated the consequences of applying Pauli's exclusion principle not only to the electrons inside the atom but to the molecules of a gas, and Dirac had shown how to formulate the principle in the new wave mechanics in terms of the symmetry of the wave function describing many identical particles, so that it was possible to apply it even to situations when the interaction between the particles is so important that one may not assign individual orbits or quantum states to each particle.

Pauli applied Fermi's generalization of the exclusion principle to the conduction electrons of a metal, and in particular to their behaviour in external magnetic fields (22). The magnetic behaviour of metals was then one of the puzzles in physics. If each electron carried a spin, as was by now established, a magnetic field would tend to orient the spins in one direction; the distribution of spin directions should obey Boltzmann's law. This would lead to a paramagnetic susceptibility following Curie's law, giving a susceptibility inversely proportional to the absolute temperature. Normal metals did not behave in this way, and Pauli pointed out that this was explained by the operation of the exclusion principle or Fermi statistics, since the electrons were normally distributed over a wide range of states of orbital motion, each containing two electrons of opposite spin. The exclusion principle does not permit the spin of an electron to change unless it is also moved to another orbital state not already occupied by an electron with the new spin direction. Hence, spin reversal must be accompanied by a considerable increase in kinetic energy, except for those electrons which could find an unoccupied orbit of about the same energy as they had before, and this can apply only in an energy region of an extent of kT, near the highest occupied state.

This result explained both the order of magnitude and the absence of a strong temperature dependence of the paramagnetic susceptibility of normal metals, and stimulated therefore further research into the electron theory of metals, which developed with impressive speed over the following years.

Up to this point the spin of the electron had remained outside the quantum mechanical description, except insofar as it allowed a doubling of the states counted for the exclusion principle and in allowing for the spin moment and its interaction with an external field of given direction. Pauli showed how to incorporate the spin dynamics into the Schrödinger theory by extending the wave equation to a pair of simultaneous equations for one electron, the two wave functions representing the amplitudes for the two spin orientations (23) This paved the way to applications involving variable fields and interactions between spinning electrons. This equation was later shown to represent the non-relativistic limit of the wave equation of Dirac, in which spin was shown to occur naturally in at least one form of the relativistic quantum theory of a single particle.

Pauli was always interested in the relation between matter and radiation (12, 14.) The similarity between light beams and beams of particles, which had been vital both in the early work on quantum theory and in the de Broglie–Schrödinger formulation of waves, had not yet found full expression in the formalism of quantum mechanics. In an important paper with Jordan (25), Pauli showed how to formulate the connexion between field and particle aspects for electromagnetic radiation in the absence of charged particles, and later in collaboration with Heisenberg (26, 27) he made the first attempt to give a full quantum treatment of the interaction of radiation with matter. These papers are among the earliest contributions to what is now known as quantum field theory. The classical theory by Lorentz of the interaction of a particle with its own electromagnetic field had led to difficulties because the field energy of a point charge was infinite and the concept of an electron of finite extent, which was favoured by Lorentz, was not easily reconciled with relativity. There seemed hope that the new concepts of space and time, which were used in quantum mechanics, would alter this situation. But the conclusion of Heisenberg and Pauli was that the self energy remained infinite. This difficulty remained a serious obstacle until in the post-war years the ideas of 'renormalization' showed at least how to obtain unambiguous answers from electrodynamics in spite of the infinities. It remains yet to be seen whether further drastic modifications of the basic concepts are necessary before we have a completely consistent theory.

Apart from drawing attention to this difficulty, the paper by Heisenberg and Pauli raised a number of points that have remained of importance in quantum field theory, particularly the part played in electromagnetic problems by gauge invariance.

In the early nineteen-thirties quantum mechanics had become an established discipline, which led to definite answers in almost all problems of atomic structure and properties, and an extensive literature developed rapidly in which the consequences of the new mechanics for a large variety of practical problems were studied. While Pauli was interested in these applications, he remained concerned with the basic structure of quantum mechanics; his article on the principles of wave mechanics (100), published in 1933, was invaluable to many of the new recruits to quantum theory, who were anxious to get a clear view of the fundamental principles. This article again shows the characteristic attitude of Pauli not to be satisfied with superficial plausibility but to think out clearly all the connexions between the different ingredients of the theory and to face the logical difficulties. Like his earlier article on relativity, it has stood the test of time, so that it could be taken over with minor changes into the re-edition in 1958 of the *Handbuch der Physik*.

Another major contribution of the pre-war period was the paper with Weisskopf (37) on the quantum theory of particles without spin or with an integral spin. The success of Dirac's work on the relativistic wave equation had left the impression that only a particle of spin $\frac{1}{2}$ (or possibly more generally of a half integral spin) allowed a consistent relativistic quantum theory; the main point being that the Klein–Gordon equation for spinless particles had no room for a particle density which was capable of only positive values. Pauli and Weisskopf tackled this problem afresh, starting from the fact that a relativistic quantum theory of photons was perfectly possible and that these were particles of integral spin. They showed that the same approach could be extended to charged particles, provided one gave up the one-particle description and incorporated in the theory processes in which the particle number could change. In that way one avoided the need for defining a particle density in space. For the interaction with the electromagnetic field it was essential to introduce a charge density, but this need not necessarily be positive if particles of both positive and negative charge were contained in the field. The absence of a positive particle density thus was seen to be the counterpart of the absence in Dirac's theory of a positive energy density. There the re-interpretation of negative energy states in terms of anti-particles ('hole theory') also led to a formalism in which a consistent relativistic description required the inclusion of pair creation and annihilation. These conclusions, which at the time appeared academic, were an essential requirement for dealing with the theory of mesons, which are today known to possess zero spin.

At about this time Pauli made a remark which must rank as one of his most important contributions to physics, even though it was not at the time published as a paper. This is the suggestion of the neutrino hypothesis. At that time one of the difficulties of nuclear physics was the existence of beta decay, with the emission of electrons with a continuous spectrum. In each individual event therefore the electron energy could not equal the energy difference between the initial and the final nucleus, and careful work had shown that this energy difference was equal not to the average but to the maximum of the electron energy, so that in each event an unpredictable amount of energy appeared to be lost. At the same time observations on spins of nuclei had made it clear that the nuclear spin was always consistent with the nucleus consisting of neutrons and protons, so that the spin was integral for even mass number and half integral for odd mass number. Since in beta decay the mass number does not change, the half integral spin of the electron cannot be compensated for by a spin change of the nucleus, and it can of course not be balanced by the orbital moment of the electron, which is always integral. The study of symmetry effects in band spectra of molecules had also shown that the statistics or symmetry of the wave functions supported the empirical rule that Fermi–Dirac statistics (i.e. the exclusion principle) applied to particles or systems of half integral and Bose–Einstein statistics to the case of integral spin. The violation of spin conservation thus also corresponded with a change in the statistics of the whole system.

Pauli postulated the existence of a neutral particle of low mass but with spin $\frac{1}{2}$, for which, to distinguish it from the heavier neutron, Fermi coined the name neutrino, using the Italian diminutive. It was evident that this postulate could restore the conservation of energy, spin and statistics. Later, Fermi showed how to formulate a theory of this process in quantitative detail and that it led to predictions for the energy distribution of the electrons and for the decay constants of beta emitters in terms of one single fundamental constant, which have been well confirmed by experiment. The idea also encouraged experiments in which one looked for the recoil of the decaying nucleus. These showed that the amount of momentum, apparently lost in the process, was connected with the loss of energy in the right way for interpreting both as the momentum and energy of an undetected particle of negligible rest mass. More recently, the ingenious experiments of Cowan and Reines, which led to the detection of inverse beta decay, have given a final proof of Pauli's hypothesis.

Pauli remained concerned about the part played by the exclusion principle as an apparently *ad hoc* addition to the rules of quantum theory and about the empirical rule which connected the statistics with spin. He showed (51) that, while an attempt to incorporate in theory particles with integral spin obeying the exclusion principle, or particles of half integral spin obeying Bose–Einstein statistics, did not lead to mathematical inconsistencies, it would lead to physically unreasonable consequences. The reason is that the elimination of negative particle densities or negative energies, which arose in the wave equation for integral and half integral spin respectively, could only be carried out by the procedures of the Pauli–Weisskopf theory or the 'hole theory' respectively if the spin and statistics were related according to the empirical rule.

In the years from 1940-45 at Princeton, Pauli's main interest was the problem of meson theory (53-55, 101). He followed the ideas of the 'strong coupling' hypothesis. This formed the extreme opposite to the picture of weak coupling, which had in general terms been successful in electrodynamics, but had led into serious difficulties in meson theory. This culminated in the paper with Dancoff (54), which solved the strong-coupling problem for the case of charged pseudoscalar mesons, leading to an isobaric state, i.e. a bound state of a meson and nucleon, of spin 3/2 and isotopic spin 3/2. We know today that the coupling between nucleons and mesons is not strong enough to justify this extreme limit, but nevertheless the result has acquired special significance through the discovery of a 'virtual' or resonance state of spin and isotopic spin 3/2 in the meson-nucleon interaction. How far this justifies the use, at least in part, of the strong-coupling picture remains to be seen.

He followed with great interest the development of quantum electrodynamics and, for example, helped to clarify the difficulties connected with the 'infra-red catastrophe', which gives rise to apparent infinities in the theory at the long wave-length end (44, 63). This is now understood, in part as a result of Pauli's work, as representing simply the fact that any collision involving a charged particle always results in the emission of an infinite number of photons of very low energy, though the total energy of all these photons is small. The natural approach of the ordinary theory in which one would calculate the probability of a collision in which no photon or only a small number of photons is emitted is therefore inappropriate. The more relevant question is to find the probability of the particle being scattered into a certain direction with its energy lying within a small interval. To this question the theory gives a finite and sensible answer.

Later the work of Tomonaga, Schwinger and others showed how to reinterpret the divergent results of quantum electrodynamics, so as to obtain reasonable answers. Pauli helped to clarify the structure of the new theory by careful analysis of the definition and structure of the various propagators which appeared in the new theory, and also (with Villars) by introducing the concept of 'regularization' (68). He realized the inadequacies of the new formalism and examined attempts to get over them by using non-local theories (76) and by widening the interpretation of the theory so as to allow negative probabilities, as in the particular model proposed by Lee (82).

He was as excited as any physicist about the discovery of violation of conservation of parity, and helped in the elucidation of the two-component theory of the neutrino (88). This is equivalent with the hypothesis that a neutrino had always a definite 'helicity' i.e. that the component of its spin in its direction of motion could have only one sign. This hypothesis led to an equation which in its mathematical form had already been postulated many years ago by Weyl, in an attempt to avoid the trouble with negative energies. Pauli had analyzed this equation in his 1933 article and had rejected it because, while not eliminating the negative energies, it violated the principle of the conservation of parity, which at that time one saw no reason to renounce.

This brief survey of Pauli's major contributions has, of course, omitted reference to many papers concerned with more detailed points and with less topical subjects. But in addition we must refer to another aspect of Pauli's role in modern physics, through his participation in discussions and, above all, in correspondence. The neutrino hypothesis, which was put forward in private discussions and in letters, has already been mentioned, but it would be impossible to list all the ideas, constructive or critical, by which he has influenced the work of pupils and colleagues in innumerable letters. Some of these letters are written in reply to requests for advice. Others were spontaneous and written either by way of comment on somebody else's work or when he had arrived at some new thought himself and just sat down to put these thoughts to somebody who he knew would be interested to hear of them. All of his pupils and friends are familiar with these letters, invariably written by hand, invariably relating to problems of crucial importance at the time, pungent in criticism. In these letters, as well as in conversation, he would often discuss conjectures and intuitive judgements, which went far beyond anything he would regard as worthy of publication, but he would draw a clear distinction between knowledge and conjecture.

To quote a very charming remark from one of these letters, he had expressed the opinion that the experiment on parity violation would give a negative answer, and had offered to bet substantial odds on this. Shortly after this he heard that the experiment had indeed established parity violation, and in a letter to V. F. Weisskopf he remarked that he was lucky that the bet had not been taken, because he would have lost some money, which he could not afford, whereas as it was he had only lost some of his reputation, which he thought he could afford.

These letters supplemented the profound influence he exerted personally on his pupils and collaborators and on many others who came to him for advice. To discuss some unfinished work or some new and speculative ideas with Pauli was a great experience, because of the clarity of his understanding and of his high standard of intellectual honesty, which would never let a slipshod or superficial argument get by. At critical times in physics, when it was not clear whether some new ideas should be taken seriously, one tended naturally to ask 'What does Pauli say about it?'

On some occasions he was hard to convince when the advocate of a new idea had not yet examined all its aspects and did not have answers ready for all possible objections, but usually he was quick to see the important truth through some unimportant inaccuracies of detail and would use his sharp criticism not to discourage but to force one into facing the objections and finding an answer to them.

He made on his own work the same, or higher, demands of soundness as on the work of others. He would never be satisfied with an argument, let alone publish it, before he had achieved perfect clarity about its basis. As a result his published work is not only remarkably free from errors (I am aware of only one incorrect published statement under his name, and even that occurs in the summary of a short talk given at a meeting (19)), but they became extremely lucid in presentation because of the amount of thought that had gone into clarifying the basic ideas.

While these qualities made him a very powerful teacher at the research level, he was not specially distinguished as a lecturer to undergraduates or to more general audiences. His elementary lectures were well thought out, but he did not have too much sympathy with the difficulties of the weaker students or the patience to go back to first principles in talking to a general audience. Probably the oversimplification and glossing over of complications, which is often essential to explain a subject briefly to a non-specialist audience, were against his nature. In lectures to groups of experimental physicists his mathematical ability would soon outrun the capacity of the listeners.

While so far only his thoughts on physics have been mentioned, it should not be thought that his interests were confined to his own subject. He was keenly appreciative of art, of music, and of the theatre, and he had a discriminating taste which would be as impatient of the second-rate in the arts as in physics. He had many close friends in the world of arts with whom he corresponded or talked about their fields as seriously as he could debate his own subject.

He was specially interested in philosophy, to which he was led both from the border-line where it impinges on physics and which was familiar to him through his thoughts on the basis of quantum theory, and from a very personal interest in psychology, and much of this interest was stimulated by (or perhaps responsible for) his friendship with the psychologist Jung.

In appearance he was a most unusual figure, a round face and a well-rounded body, with rather awkward movements and a very characteristic habit of rocking his body, particularly when in thought ('Wenn er mit Problemen kämpft, ist die Schwingung ungedämpft' read an irreverent verse about him at one international conference). He was not good with his hands, or with mechanisms, which he tackled with a careful and suspicious deliberation. He passed his driving test after 100 lessons, but did not drive much afterwards.

One can see here the probable origin of the legend of the 'Pauli effect'. This was a kind of spell he was supposed to cast on people or objects in his neighbourhood, particularly in physics laboratories, causing accidents of all sorts. Machines would stop running when he arrived in a laboratory, a glass apparatus would suddenly break, a leak would appear in a vacuum system, but none of these accidents would ever hurt or inconvenience Pauli himself.

At one reception this Pauli effect was to be parodied, and a chandelier had been suspended carefully by a rope which was to be released when Pauli entered, causing the chandelier to crash down. But when Pauli came, the rope became wedged on a pulley and nothing happened—a typical example of the Pauli effect!

Anyone who met Pauli for the first time would have been tempted to class him as an absent-minded professor, but such a judgement would have been wide of the mark. He had in fact learnt to arrange his affairs and his way of life so as to interfere least with his work and with the things to which he was inclined to devote his time. He had no patience for conventions, unless he saw some sense in them. His ways of going about practical things were often unorthodox, but they generally achieved the object of saving time and effort and concentrating on what he regarded as essential. One aspect of this was that he was selective in the type of problem in which he was prepared to take an interest; he could be very discouraging to colleagues who wanted to arouse his interest in problems which he regarded as too technical or too complicated or just too uninteresting. This would be done with a disarming frankness. He generally avoided meetings or discussions on subjects that bored him, but, if he did find himself at such a meeting, he was apt to announce cheerfully that he did not know anything about the subject and thought it dull.

But this is not to say that his interest was restricted to fundamental problems. He knew well that big things are often made up of many points of detail, and he could be extremely patient in discussing minor points once he regarded them as relevant. Similarly, his interest in people was by no means confined to the great authorities. He was extremely interested in young people and their problems, and, if people were in difficulty with their personal problems, he could be most patient and helpful in listening and advising.

On one occasion he was talking with a group of young people, who were young enough to think they knew everything. Sensing that they were not inclined to listen to him he said, 'I have more experience than you. I have once been young, but you have never yet been old.'

No account of Pauli and his attitude to people would be complete without mention of his critical remarks, for which he was known and sometimes feared throughout the world of physics. He not merely did not spare the other person's feelings but he often deliberately selected the sensitive spot.

No doubt many of the stories of this kind circulated about him are apocryphal, but the examples below come from reliable sources or from conversations at which the writer was present. The oldest of the famous remarks dates back to the Munich days, when Pauli was a brilliant but unknown research student, and at a crowded colloquium meeting Einstein, who was visiting, made a comment in the discussion. Young Pauli rose at the back of the hall and said: 'You know, what Mr Einstein said is not so stupid', a remark characteristic for his lack of respect for authority but not yet of the bite which came later with his greater assurance.

N. Kemmer reports a more characteristic remark. 'I do not mind, Mr X, if you think slowly, but I do object when you publish more quickly than you think.'

When L. Landau, after a long argument in Zurich, pleaded for an admission that not everything he had said was complete nonsense, Pauli replied, 'Oh, no. Far from it. What you said was so confused that one could not tell whether it was nonsense or not.'

When a charming colleague whose papers had not impressed Pauli had given him directions how to find a certain place in a strange town and enquired the next day whether Pauli had found the place, he said, 'Oh, yes. You express yourself quite intelligibly when you don't talk about physics.'

Quite recently, a friend showed him the paper of a young physicist which he suspected was not of great value but on which he wanted Pauli's views. Pauli remarked sadly, 'It is not even wrong.' People have tried to attribute these sharp remarks to Pauli's impatience with slipshod reasoning and wishful thinking. There is no doubt that he was using them as a tool to drive home valid and often constructive criticism, but equally often they were so remote from any specific point in the argument that it is doubtful whether this is the full story. He himself once said to the writer, 'Many people have sensitive corns and the only possible way of living with them is to step on these corns often enough until they get used to it', but that remark, too, probably oversimplifies the problem.

The remarkable thing is that, although the victims often felt hurt at the time, none of then ever bore a grudge for long. It is a tribute to his greatness as a physicist and as a man, and to his understanding of other people, that all who knew him, who all must at one time or another have been exposed to remarks of this sort, had as much affection for him as they had respect for his knowledge, his judgement and his integrity.

His death was quite unexpected. Few people knew that he was unwell and he himself did not realize the seriousness of his condition. Until almost the last day he would not let discomfort and severe pain interrupt his routine of work and of thought. But the feeling of loss caused amongst the family of physicists by his death was so severe, not only because of the suddenness of the event but because of the unique position he held in physics as a colourful personality, as a thinker of unusual clarity, as a powerful critic and adviser, and as representing through his example and through his criticism what has been called the conscience of physics.

The writer would like to acknowledge his indebtedness in writing these notes to many colleagues and in particular to Mrs Pauli and to Mrs Hertha Pauli-Ashton. He owes the appended list of publications to Dr Charles Enz of Zurich.

R. E. Peierls

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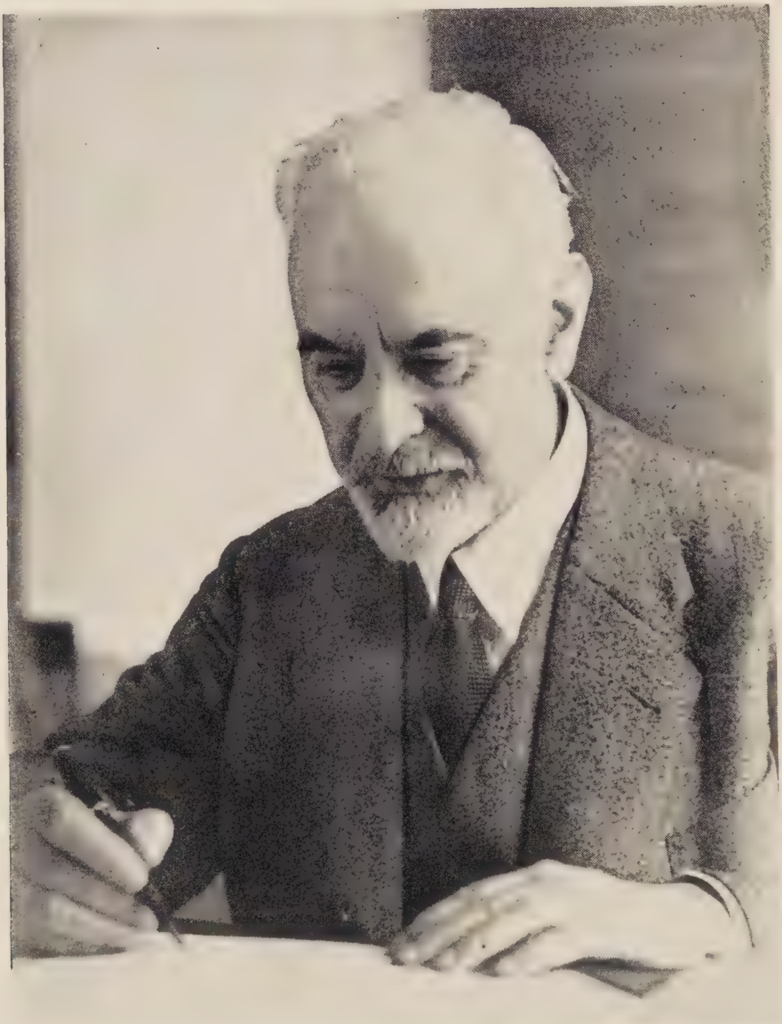

L. Prandtl.

LUDWIG PRANDTL

1875-1953

It is a tragedy in our age of technological evolution that those who initiate progress in important fields so often live to see their results applied to quicker and more thorough destruction. Certainly aviation is one of those dubious blessings. And the father of aerodynamics, Ludwig Prandtl, familiar since his youth with treasures of art and architecture in the lovely old city of Munich, doubtless could not help being haunted by the thought of why the day, on which mankind learns how to fly, must be the last day for many of these creations.

In heavier-than-air aviation, which Prandtl advanced by advocacy of wide-span monoplanes, he was not himself a pilot, while in the lighter-than-air department, in which he fathered the streamline shape of dirigibles, he was at least a licensed practising balloonist. But he was not even a passenger in an airplane until about 1930, and his aerodynamical laboratory flourished without any connexion to an airfield. Prandtl preferred a small staff of rational thinkers and was almost afraid of the emotional interference which comes with the touch of the stick and with real flying. In no way does that mean that all play and fun were taboo at work. On the contrary, Prandtl's own way to tackle the Navier–Stokes equations was to toy with flows far beyond the range of practical application, showing the pleasure and also the marvelling observation of a child, till a phenomenon crystallized that asked for explanation. Spheres and cylinders were used as models—there would be no 'Karman vortex street' without that—sharp-edged corners were preferred to complicated shapes. He liked to reduce the number of arbitrary shape parameters, etc., so the brain would have no excuse for misinterpretation. The growth in time of starting vortices or of the curling ends of discontinuity layers, or the steady supersonic flow around the corner, are famous examples without any geometrical length, as indeed is the development of the boundary layer from the edge of a flat plate.

Prandtl was born on 4 February 1875 in the Bavarian town of Freising, the only remaining child of three born to Magdalene Ostermann and Alexander Prandtl, professor of surveying and engineering at the Weihenstephan agricultural college. He learned Latin and Greek at a high school in Munich before he entered college to become a mechanical engineer. During his college years he lost both parents. In Munich at that time was the famous professor of engineering mechanics, August Foeppl, who was a revolutionary spirit engaged in rationalizing continuum mechanics, and a rival to the school of C. Bach, the more conservative group dominating the German engineering society. There was no doubt that Prandtl felt at home in Foeppl's laboratory, when in his doctor's thesis, appearing in 1900, he tackled the torsional instability of beams with an extreme depth-width ratio. This paper and his soap-film analogy for the torsion problem, with its immediate recognition, could have been the start of a career in elasticity, but this was not his fate, though, as a matter of record, he never abandoned this subject nor the subject of plasticity.

A freak assignment, while Prandtl was employed at the firm, M.A.N., at Nuernberg, was to become the turning point in his life. He was told to investigate the failure of the pneumatic chip-removal installation in the carpentry shops. With his background in rational mechanics, the pneumatic installation, designed by empirical hydraulics, was bound to be a challenge for a young engineer who had not yet felt his limitations. His investigation was so successful that the firm not only reconstructed its installation accordingly but offered similar installations as a new line of products.

Soon he left this job for a start in his father's beloved academic career, when called as professor of mechanics to Hannover in the autumn of 1901. The 26-year-old Prandtl, however, did not give up his studies aimed at placing fluid mechanics on a rational basis. In a manually-driven portable water channel of a do-it-yourself type he observed flows and took pictures with one of the early photographic cameras and within four years he had elucidated one of the great riddles of flow with vanishing viscosity—the boundary layer, or, in Prandtl's original terms, the friction layer on the walls. He presented this result at the Third International Congress of Mathematicians at Heidelberg in 1904.

While Prandtl's entry to his lifetime task was admittedly accidental, his further climb on the ladder of success was comparatively logical, but there were still unusual circumstances which brought him to the place of his lifetime work. The mathematician and editor of the *Encyclopedia of Mathematical Sciences*, Felix Klein, famous for his unifying mathematical concepts, for influencing the teaching of mathematics in schools, and for tracking down the sources of mathematics wherever they may be, had the idea that the University of Göttingen needed a cross-breeding of its pure mathematical tendencies with applied sciences in order to keep up its traditions. So he brought four strangers, men of chemistry and engineering, to a place of worship of the humanities, which meant the breaking down of quite a few fences. Time proved all of these men to be well chosen, and Prandtl was one of them. In the autumn of 1904, he was made head of the applied physics—later to be called applied mechanics—laboratory. Prandtl's own students were from then on majoring in mathematics, men like H. Blenk, O. Tietjens, W. Tollmien, and H. Schlichting. The vital construction engineers for his wind tunnels, like G. Fuhrmann, A. Betz, M. Munk, C. Wieselsberger, J. Ackeret and many more, were usually hired on recommendation or earlier acquaintance.

The Wright brothers having already succeeded in their first flight, and others soon afterwards, the era of flight had begun and the necessity to develop the aeronautical sciences became apparent. In 1906 a study committee was set up by proponents of dirigibles and, with Felix Klein as spokesman, plans were presented to create a model testing laboratory at Göttingen. The tool for such tests, a 35 h.p. wind tunnel, was built in 1908. This was the first offspring of the do-it-yourself water channel; it retained the turning vanes in the four corners of its closed circuit, which is one of the family traits of the Göttingen-type tunnels. The velocity distribution in the test chamber was subsequently improved by the incorporation of a large contraction, following a settling chamber with honeycomb, so familiar in conventional tunnel design. In 1915, during World War I, another wind tunnel project was started and completed in 1917, with 300 horse-power. In 1926, a third wind tunnel for propeller research, with increased velocities at reduced pressures, was built on the same location, while in 1933, a full-scale wind tunnel followed.

Wind tunnels of larger size are beyond the budget of a university laboratory and require sponsors for their building and an organization for their operation. Prandtl was lucky to find such sponsors and vice versa. After the study committee for dirigibles had completed its task and dissolved, the Kaiser Wilhelm Society for the Advancement of Science was prevented from creating a large aerodynamical centre only by the outbreak of the first world war. Soon after, however, by a joint effort of army and navy during the war, the laboratory was organized and the staff recalled from the armed forces. After the war a private organization of sponsors of the Aerodynamical Laboratory took responsibility and found support from the Ministry of Transport while, after 1933, more substantial support came from the research department of the Air Ministry. Prandtl, whose endeavour it was to put aerodynamics on a rational basis, wondered whether theory would do the main job and wind tunnels be for spot checks, or whether the tunnels would still remain his most successful children. Timetables of the total horse power installed in wind tunnels around the earth were sometimes drawn in Göttingen for the past, and extrapolated with a daring exponential curve into the future; but it seems that during Prandtl's lifetime the extrapolations into the future proved always below the actual growth.

Prandtl's favourite type of research was more closely realized in 1925 when after the years of inflation the recovered Kaiser Wilhelm (later named Max Planck) Society for the Advancement of Science succeeded in surrounding Prandtl with the proper equipment. The new laboratory for flow research adjacent to the wind tunnels contained on a larger scale facilities for supersonic flow in air and for cavitation tests in water. They consisted mainly of a couple of tanks, a gasometer for dried air with the drying equipment, and a compressor, a vacuum pump and a water pump, to continue the study of diversified flow phenomena so successfully started in 1904 to 1908 in the applied mechanics laboratory of the University. His meteorological interest was supported by a rotating chamber for flow observations. When, therefore, at about the time of the Volta Conference in Rome, 1935, the compressibility effects at high-speed flight became a matter of concern, Göttingen was again well prepared with results, like the Prandtl–Glauert rule for subsonic flow and the characteristics method for supersonic flow, which Prandtl had used in a primitive form (only valid for perfect gases at $\sqrt{2}$ times the sonic velocity) as early as 1906 to shape the exit of his Laval nozzles for parallel supersonic jets—work which was laying the foundation of compressible aerodynamics as surely as his work on boundary layers was building up viscous flow theory.

Prandtl's scientific achievements in aerodynamics start with the proof that the Navier–Stokes equation for small viscosity leads to solutions in which the viscosity is relevant only in a thin layer surrounding solid walls, the so-called boundary layer. Separation of this layer from the body forms wakes and causes large pressure drag. Proper streamlining of the body reduces the drag to a mere shear drag if the boundary layer can be kept on the body till it unites from all sides at the rear. This insight into the causes of drag makes streamlining a rational operation and explains the stalling at larger angles of attack of wing profiles. Two other phenomena became thereafter ready for a full explanation. Streamlined wings of finite span showed a drag due to lift, which Prandtl called induced drag after being able to determine its numerical value and the lift distribution to minimize this drag. His basic concepts are the same as in Lanchester's vortex theory of lift. But Prandtl's trick, to start with small lift values on a lifting line normal to the flight direction, avoids most of the difficulties encountered in the general problem. While the separation of boundary layer has generally only a minor dependence on the Reynolds number, discontinuous changes were observed by Eiffel and others for the drag of spheres, cylinders, and for the stalling angles of wing profiles at a Reynolds number of the order of half a million. Prandtl was able to explain this critical point by the alternative possibilities of laminar or turbulent boundary layer flow, similar to Reynolds's laminar or turbulent pipe flow. Since the boundary layer thickness grows with the square root of the body length, the order of a million reduces to the order of a thousand for a comparable pipe flow, which is the familiar order of Reynolds numbers for flows becoming turbulent in channels and pipes.

The relation to turbulent pipe flow, discovered as early as 1913 and used qualitatively to explain the similarity between shear and heat transfer along the surface, induced Prandtl later to investigations about the origin of turbulence. Though a self-supporting turbulence is strictly a three-dimensional phenomenon, the instability for small disturbances, if it exists, can be investigated by superposition of two-dimensional particular solutions. Prandtl, whose own doctor's thesis was about elastic instabilities, had a number of his Göttingen students, O. Tietjens, W. Tollmien and H. Schlichting. and also A. Sommerfeld's pupil W. Heisenberg, working on the hydrodynamic instability of boundary layer profiles for their theses. H. B. Squire, a guest from England, completed the three-dimensional aspect by adding oblique disturbances. The work itself proved successful though it is on the border line where those two necessities, permissible simplifications and convincing rigor, might not readily be agreed to by a sceptical audience. Thus it was not until after the second world war that this chapter of smooth wall turbulence found international recognition along with the other possibility of starting turbulence at finite amplitudes with the help of roughness or other imperfections.

During this time Prandtl worked simultaneously on the problems of fully developed turbulence in wakes by introducing the concept of mixing length. He also generalized the knowledge of criteria for separation, by comparing a compressible flow to an incompressible flow with similiar pressure distribution. The equivalence of a thin compressible and a thicker incompressible flow of reduced scale in the crosswise direction is the so-called Prandtl–Glauert rule.

Looking at Prandtl's aerodynamic achievements, which he presented in his steadily expanding book, starting as two handbook articles in 1913 and ending as *Essentials of fluid dynamics* in 1952, and considering the many other subjects of his publications, one feels small wonder that his greatest enjoyment came from his work and the discussion of problems with pupils, whose number and variety of background increased with the growth and fame of the laboratory. Special studies at the laboratory were performed by foreign guests; some English guests were J. W. Maccoll, S. Goldstein, H. B. Squire, and Miss L. M. Swain. Nevertheless his tour around the world in 1929 on the occasion of the World Engineering Congress at Tokyo and the reception by his friends in foreign countries were highlights in his career.

Prandtl as a person was a modest, almost shy, man who regarded his gift, of such amazing clarity in dissecting and composing mechanical phenomena and finding ways to reduce their complexity, as a grace not as a gold mine. The influence, in developing this gift, of his famous teachers, August Foeppl and Felix Klein, resulted in everlasting gratitude. He asked for the hand of Foeppl's eldest daughter, Gertrude, in 1909; he was then well prepared to support a wife, and the two daughters which came in 1914 and 1917. His refusal of a call to Foeppl's chair in Munich, 1923, in favour of a new laboratory in Göttingen, is an example of the traditional method of trading in academic life, and is the reason for many excellent laboratories in Germany. Prandtl loved his native state, Bavaria, and hiking in the hills around Göttingen was only a weekend substitute for the mountains of his summer vacations in Bavaria. He was musical, and calibrated his absolute pitch while a member of an orchestra in his youth. His private life and his family life were inconspicuous except for the annual party which he enjoyed giving in later years for his laboratory staff. His pupils found Prandtl's work and life such a complete unity that they instinctively oversimplified his personality, as is revealed by the few anecdotes which claim his reactions to be predictable. Of course, he could hardly pass a toy without touching it, and to figure out how a magician's trick works was to him just as serious a problem as an unbelievable result in a test. He was a passionate teacher, but in spite of all his admiration for witty remarks of others, he preferred to make the ideas shine, not his words.

Prandtl's publications share the authorship with his pupils. He would be more tempted to write for them than to have their contribution incorporated in his papers, even if it were one as personal as an anniversary contribution. He observed rigid rules of personal integrity which were not open to anybody's advice. Thus he could surprise his friends in small things, like refusing his signature on an application for extra food rations for his laboratory, as long as his name was part of the list. And he could surprise them in serious things. When in the laboratory, one professional party speaker finished his standard speech by saying that nothing positive was done in the past fourteen years except the marching of the brown columns, Prandtl could jump up and summarize in an eloquent little address for the members of his staff just those few important positive things done in aerodynamics within the last fourteen years by people who also love their country. Such talk and the following applause could ruin a man. It took some quick thinking of his friends to make Goering allocate money for a new wind tunnel, to get Field Marshall Milch to present it as an anniversary gift to Prandtl, and to assure the irreversibility of his honour by a torch parade of the students to his residence.

Commonly Prandtl's talk was not quite so eloquent as in times of emotion. According to his aim to make his sentences foolproof, they required a re-wording, a re-phrasing, an explanation of the reasons for re-phrasing and perhaps the discussion of some exception to the stated rule. It is quite obvious how different the result of such lectures must have been for beginners and for listeners with background. Seeing the details as clearly as Prandtl did, and never trying to circumvent difficulties by omitting them, made his lectures, seminars, colloquia a rich source of information for all his pupils, besides those fortunate few who walked with him home from work. Spotting at the gate the group bringing each other home did not mean a thing, as the wives had to learn: it still was not sure at which house the discussion would stop, to be carried on by a smaller group with fewer residences as points for further decrystallization. This is the secret behind handbook articles offered to Prandtl but written by his pupils. Prandtl's own work was done from supper to midnight in the study of his rented flat. Late in the morning after classes, or direct from home, he came with new results or with ideas not quite ready, which he would like to discuss.

The second world war took much of his time for the many obligations which he had to accept because of his knowledge, or because his reputation might shield others, when the war machine faltered. Among all those creations, the Academy of Aircraft Research with its monthly meetings was one institution in which Prandtl was again receptive and productive to the delight of his former pupils. Giving from his large store of experience thoughts and details worthy of preservation for posterity, made him seem to enjoy these meetings just as much as the other members.

When his first grandchild died, when his first son-in-law was killed during the war, and when his wife died immediately after the war, these events and the general situation of his creations diminished his vitality to a dangerous degree and friends in Switzerland invited him to a successful recovery. His work, the nerve of his life, was continued with new editions of his book and a number of papers on different subjects. His own favourite is the explanation of 'Weather phenomena in the upper troposphere' of 1949. But his body lost its strength and, his light being already dimmed during his last year, death came on 15 August 1953.

Prandtl received many honours and medals (some British ones are the honorary doctor's degree from the University of Cambridge in 1936, foreign membership of the Royal Society of London from 1928, and honorary membership of the Royal Aeronautical Society and receipt of its gold medal). They made him happy when they arrived, though the recording of this happiness he left to his wife. His gift, combined with his modest personality and his pleasure in helping others, made him many real friends. Only a very few were able to return him a favour, and nobody as much as Albert Betz, his associate for about forty years. The rest will be fortunate if they succeed in giving to others as much as they received from him.

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O.W. Richardson

OWEN WILLANS RICHARDSON

1879-1959

Owen Willans Richardson, one of the most outstanding men of science of this century, was born on 26 April 1879 at Dewsbury, Yorkshire; the son of Josiah H. Richardson and Charlotte M. Richardson. Most of his early childhood was spent at the little mining village of Askern near Doncaster and both his parents are buried there. He was a very precocious boy and gained so many scholarships and exhibitions that his education cost his parents nothing at all.

He was a pupil at Batley Grammar School till about 1897 and there is an entry at the school, in the Register kept for the 'Department of Science and Art', which, as many older men will remember, used to hold examinations all over England: 'O. W. Richardson, age last birthday 13, date of entering school September '91. Address Victoria Crescent, Dewsbury. Father's occupation: Traveller. Signed O. Richardson.'

According to the present Headmaster, Mr F. W. Scott, Batley Grammar School was founded in 1612 and built beside the Parish Church, but moved up the hill in 1878 to new buildings. It has produced many learned men and scientists; but like many other schools (or indeed most schools) it suffered severely from the tyranny of examinations, that bane of education in England.

From Batley Grammar School young Richardson proceeded, with a scholarship, to Trinity College, Cambridge in 1897. In the Natural Sciences Tripos Pt. I he obtained a 1st class with the highest possible distinction in physics, chemistry and botany and he once said that it was a toss-up whether he should proceed further with physics or with botany. In this he seems to have resembled the great mathematician, J. W. Nicholson (later one of his colleagues at King's College, London) whose biological interests were very strong in his youth. Richardson obtained a 1st Class in the Natural Sciences Tripos Pt. II and the B.A. degree in 1900. He became a Fellow of Trinity in 1902 (Honorary Fellow in 1940), Clerk Maxwell scholar and D.Sc.(London) in 1904.

At Cambridge he was one of a remarkable group of young men working at the Cavendish Laboratory under the inspiring leadership of J. J. Thomson (later Sir Joseph Thomson, and President of the Royal Society). Among those who were his contemporaries, or who were at Cambridge about that time were: C. T. R. Wilson, of Cloud Chamber fame, who is still happily with us, and Harold A. Wilson. The former would be about 10 years his senior and the latter about 5 years. Among others who were in Cambridge during or near Richardson's time were: the great Rutherford, Soddy, Aston, and J. W. Nicholson.

The short period which began about 1887 and continued till about 1930 was one of the greatest in the history of physical and cosmological science. The famous experiment of Michelson and Morley was carried out in 1887—though Michelson himself had made a tentative effort at such an experiment as early as 1879—but its profound significance was not grasped till Albert Einstein began to develop his relativity theory (in 1905 and the following years). Max Planck's wonderful innovation of the elementary quantum of action came in December 1900.

Richardson's earliest work on the emission of electricity from hot bodies—he gave the name thermionics to the subject—was carried out on platinum since it had a higher melting point (1755 °C) than anything else at his disposal at that time. Later on ductile tungsten became available and enabled him to study the phenomenon of emission of electricity from hot bodies at much higher temperatures. J. J. Thomson showed in 1899 that the discharge from an incandescent carbon filament in a vacuum tube was carried by negative electrons, and in 1900 McClelland showed that the currents from a negatively charged platinum wire were influenced very little, if at all, by changes in the nature and pressure of the surrounding gas, if the pressure were fairly low. In 1901 Richardson was able to show that each unit area of a platinum surface emitted a definite number of electrons per unit time. He found that this number increased very rapidly with temperature and that the maximum current, i, at any Kelvin work scale temperature, T, was governed by the now well-known law

$$i = AT^{\frac{1}{2}}e^{-\frac{w}{kT}} \tag{1}$$

In this equation k is the so-called Boltzmann's constant—actually first explicitly introduced by Planck—and A and w are constants of the material. Later he was able to show that the same conclusions followed for other materials, e.g. sodium and carbon, and that the differences of the work functions of different materials should be equal to their contact potential differences. The main idea lying behind formula (1), as Richardson has said, is that of an electron gas evaporating from a hot source. Somewhat later certain thermodynamical considerations led to the conclusion that the formula

$$i = AT^{2}e^{-\frac{w}{kT}} \tag{2}$$

is more correct than (1); but the exponential factor in these formulae is so powerful that it is hardly possible to distinguish between them experimentally. It should be noted that equation (2) is not affected by later views (Fermi–Dirac) about the character of the electron gas *inside* the metal.

In 1906 Richardson was appointed Professor of Physics at Princeton University, U.S.A. Here, in collaboration with F. C. Brown, he showed that the distribution of velocities among the electrons emitted by a hot body was the familiar Maxwellian one. The constant w, as Richardson has pointed out, must, if Sommerfeld's theory of conduction be accepted, be identified with the difference between the work required to remove a free electron, at rest inside the metal, to a point outside and the maximum energy of the internal electrons.

Among Richardson's pupils at Princeton were the brothers A. H. and K. T. Compton. The former discovered the well-known Compton effect, namely that scattered X-rays have a slightly longer wave-length than the primary rays. Indeed, some time before Compton's discovery, Barkla had noticed that the scattered rays were more absorbable by his aluminium plates than were the primary rays. Richardson, in collaboration with K. T. Compton, verified Einstein's photo-electric law. This was done independently and about the same time by A. L. Hughes, then working, I believe, with H. A. Wilson in Texas.

Among Richardson's colleagues at Princeton were the well-known physicist C. J. Davisson and the mathematician O. Veblen, both of whom married sisters of Richardson.

In 1914 Richardson was appointed to the Wheatstone Chair of Physics at King's College, London. Here he carried out work on photo-electricity with F. J. Rogers and spectroscopic researches with C. B. Bazzoni. He also carried out experiments on the spectra of hydrogen and helium and compared the results with Bohr's theory. He predicted the gyromagnetic effect which led to the discovery of the gyromagnetic anomaly which is due to the fact that the ratio magnetic moment/angular momentum of an electron orbit is e/m_0c instead of the classical $e/2m_0c$, where e and m_0 are respectively the charge and the rest mass of the electron. It was this which led Uhlenbeck and Goudsmit to ascribe to the electron the spin momentum $h/4\pi$ (1925).

In 1924 Richardson was appointed Yarrow Research Professor of the Royal Society and from then onwards he was able to devote all his time to research. He continued to be interested in the triplet fine structure of H_2 on which he had published high resolution data. His last paper was published so recently as 1953, long after his retirement (1944) from the Yarrow professorship. He endeavoured to apply wave mechanics to calculate the cold emission of electrons from metals under intense fields and he collaborated with Dr H. T. Flint in a paper dealing with the possibility of a least *proper time*. He studied the soft-X-ray spectra of a number of metals with the late F. C. Chalklin (earlier one of his students at King's College, London) and Mrs Chalklin. He also collaborated with W. E. Williams and J. Drinkwater on the wave numbers and fine structures of H_α and D_α lines to test Dirac's theory.

In his earliest King's College days he lived in Cannon Place, Hampstead and I remember Bohr staying there as a guest. Later Richardson moved to 45 Haverstock Hill, a much more roomy house with a fine garden. He filled his house with valuable and beautiful things, especially pictures. He had many distinguished visitors, one of them being the great Arnold Sommerfeld.

Among other memories of the Haverstock Hill house is that of a dinner at which the great Hendrik Antoon Lorentz and his wife were the principal guests. At one end of the table was that delightful and gracious lady, Richardson's first wife, Lilian, a sister of H. A. Wilson, who himself had lived in Hampstead while Wheatstone Professor at King's College.

Richardson loved to have a cottage in the country. The earliest I remember was at a place called Forest Green, I think, near Holmbury St Mary and on the Surrey-Sussex border. Here he had also several acres of woodland and some ancient cottages. Later he had a cottage at Graffham, just where the Roman Road descends from the South Downs—not far from Petworth, and he and his family often went to swim in a pleasant pool in that neighbourhood. He liked long country rambles and I remember a long tramp with him along the South Downs from Lewes westwards, ending eventually at Fittleworth. This was in September 1921.

His wife Lilian died during the King's College period and he married again in 1948. His second wife, who survives him, was named Rupp and is a distinguished scientist, being one of those who had observed what was then regarded as an extraordinary phenomenon, namely the diffraction of electrons.

Richardson was knighted in 1939. He was President of the Physical Society from 1926 to 1928 and served as its foreign secretary for many years. Honorary degrees were conferred on him by St Andrews and London (LL.D.) and by Leeds (D.Sc.). He was also a Fellow of King's College, London.

In his later years Richardson lived in a pleasant house called Chandos Lodge, near Alton in Hampshire, and for some time had a farm in that neighbourhood. He was a remarkable blend of an excellent business man and a man of science. He leaves two sons and a daughter. The elder son, Harold, is Hildred Carlile Professor of Physics in the University of London (Bedford College) and the younger, Jack, is a distinguished medical man and psychiatrist in Grantham.

Richardson was a genial, pleasant and very helpful colleague and many of us, his colleagues and students, will always feel indebted to him.

It is a duty and a pleasure to acknowledge my indebtedness to his sons and others without whose help I could not have written this memoir. I am particularly indebted to Dr E. W. Forster's obituary notice in *Nature* last April.

WM WILSON

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M. N. Saha

MEGHNAD SAHA

1893-1956

THE name of Professor MEGHNAD SAHA would always remain associated with the theory of thermal ionization and its application to the interpretation of stellar spectra in terms of the physical conditions prevailing in the stellar atmospheres. The theory had all the simplicity and inevitableness which usually characterize a fundamental and epochal contribution. It was almost a direct consequence of the recognition that the laws of thermodynamics and the kinetic theory of gases can be extended to a gas of free electrons. Apart from astrophysics, the theory later found numerous other important applications, such as, to mention some of them, in the study of the ionosphere, conductivity of flames, electric arcs and explosion phenomena. Saha's researches in astrophysics and physics extended over a wide range of subjects. At one time or the other he worked on stellar spectra, thermal ionization, selective radiation pressure, spectroscopy, molecular dissociation, propagation of radio waves in the ionosphere, solar corona, radio emission from the sun, beta radioactivity, and the age of the rocks. Besides physics he took a keen interest, at times almost bordering on the professional, in ancient history and archaeology. He was a devoted and inspiring teacher, and he gave his time generously to his students. He organized active schools of research at Allahabad and Calcutta; and in establishing the Institute of Nuclear Physics at Calcutta, in building the laboratories of the Indian Association for the Cultivation of Science, and in founding academies of sciences in India, his role throughout was of the utmost importance. He, more than anyone else, was responsible in starting the monthly journal *Science and Culture*, and he was its editor for many years. He was from the beginning a member of the Council of Scientific and Industrial Research constituted by the Indian Government in 1942, and member (or chairman) of several of the research and other committees of the Council. He was the Chairman of the Council's Indian Calendar Reform Committee. He was an elected independent member of the Indian Parliament. He took the keenest interest in problems of national planning, particularly in relation to science and industry. He was an active member of the National Planning Committee appointed by the Indian National Congress in 1938 with Jawaharlal Nehru as chairman. In his criticism of things and men, Saha was fearless and trenchant, and he was motivated by a deep earnestness and sincerely, though often tenaciously, held convictions. His memory and versatility were amazing. He was extremely simple, almost austere, in his habits and personal needs. Outwardly, he sometimes gave the impression of being remote, matter of fact, and even harsh, but once the outer shell was broken, one invariably found in him a person of extreme warmth, deep humanity, sympathy and understanding; and though almost altogether unmindful of his own personal comforts, he was extremely solicitous in the case of others. It was not in his nature to placate others. He was a man of undaunted spirit, resolute determination, untiring energy and dedication. On 16 February 1956, on his way to the Office of the Planning Commission in New Delhi, he succumbed to a sudden heart-attack (some hundred yards from the Office of the Commission) and at the age of sixty-two, a career superb in science and great in its promotion and dissemination was tragically closed.

Meghnad Saha was born on 6 October 1893, in the village of Seoratali in the district of Dacca, now in East Pakistan. He was the fifth child of his parents, Jagannath Saha and Sm. Bhubaneswari Devi, who had five sons and three daughters. The family depended for its livelihood on the very meagre income from a petty shopkeeping business, and Saha's early education was beset with many hardships. In the village at that time there was no high school. Even the nearest middle school teaching English was in another village seven miles away, and Saha was able to join it due to the generosity of a local medical practitioner, Ananta Kumar Das, who agreed to provide him with free board and lodging in his house. In 1905 Saha joined the Government Collegiate School in Dacca after securing a Government scholarship for standing first, at the middle school examination, in the Dacca district. This was the year of great political unrest in Bengal caused by the partition of the province against popular opinion, and the school also was not without its share of trouble. The young Saha was drawn into a boycott of the visit of the Bengal Governor to his school, and as a sequel he forfeited the scholarship, and had to leave the Government School with many others. He now joined a private school—the Kishai Lal Jubilee School—and passed the Entrance Examination of the Calcutta University in 1909, standing first amongst the East Bengal candidates. He was a precocious student, and he was equally good in mathematics and the languages. He stood first in the all-Bengal competition examination in the Bible, open to school and college students, conducted by the Baptist Mission. In 1911 Saha passed from the Dacca College, Dacca, the Intermediate Science Examination of Calcutta University. He was first in mathematics and chemistry, but third in order of merit in the whole examination. One of his subjects at the examination was the German language which he studied privately—the college had no arrangement for its teaching.

Saha now entered the Presidency College, Calcutta. Here he had amongst his contemporaries many who are familiar names in Indian Science, such as S. N. Bose (the author of the quantum statistics that goes by his name), N. R. Sen, J. N. Mukharjee and the late J. C. Ghosh. P. C. Mahalanobis, the distinguished statistician and planning expert, was his senior by a year; N. R. Dhar was senior by two years. Amongst his teachers Saha had *Acharya*

P. C. Ray in chemistry, Jagadish Chandra Bose in physics, and D. N. Mallik and C. E. Cullis in mathematics. Both in the B.Sc. Examination (1913) with Honours in Mathematics and the M.Sc. (Applied Mathematics) Examination (1915), Saha had the second place, the first position going to S. N. Bose. He intended at one time to take the competitive examination for the Indian Finance Service, but was not granted permission by the Government. He resolved to devote himself to study and research in applied mathematics and physics. To support himself and his younger brother staying with him, he for a few months took to private tuitions of which at one time he was doing as many as three in different parts of Calcutta, covering the long distances on a bicycle. In 1916 the Calcutta University under the dynamic leadership of its Vice-Chancellor Asutosh Mukerjee, a Judge of the High Court, opened a new University College of Science for post-graduate studies and research—this was made possible because of the magnificent donations of two eminent lawyers of Calcutta, Tarak Nath Palit and Rash Behari Ghosh. Saha and S. N. Bose were appointed lecturers in the Department of Mathematics with Dr Ganesh Prasad as Professor. He, however, found it irksome to get on with the Professor of Mathematics, and in 1917 he (with S. N. Bose) was transferred to the Department of Physics. About a year later C. V. Raman joined the Department as Palit Professor of Physics.

Saha's early lectures to the post-graduate classes covered diverse subjects such as hydrostatics, the figure of the earth, spectroscopy, and thermodynamics; and he was in charge of the Heat Laboratory. Many of the subjects were new to him—he had taken physics in the undergraduate classes only—but this only served to stimulate his dauntless spirit all the more, and he studied the new subjects, specially thermodynamics and spectroscopy, with great avidity and zest. It was fortunate, as he often recalled in later life, that about a year earlier he had come across Miss Agnes Clarke's two popular books on the sun and the stars. These books fascinated him greatly and gave him some idea of the main problems in astrophysics. He now read, amongst other books, Planck's *Thermodynamics* and Nernst's *Das Neue Warmesätz*, and he was familiar with the papers of Bohr and also Sommerfeld on the quantum theory of the atom. All this in a sense paved the way for his epochal work on the thermal ionization of elements. More will be said about this presently.

Shortly after the end of the First World War there was announced the momentous discovery of the deflexion of starlight by the gravitational field of the sun, confirming Einstein's theory of general relativity. Saha got deeply interested in the relativity theory. He, jointly with S. N. Bose, prepared an English translation of Einstein's papers, later published in the form of a book by the University of Calcutta. The study of relativity led Saha to some investigations in electromagnetic theory and his first original paper entitled 'On Maxwell's stresses' appeared in the *Philosophical Magazine* in 1917. This was followed by papers on the dynamics of the electron. He derived, on the basis of the special theory of relativity, the Lienard–Wiechert potential due to a point-charge. During these years he also worked on radiation pressure, and in 1918 (with S. Chakravorti) he published in the *Journal of the Royal Asiatic Society of Bengal* (Calcutta) a paper on the measurement of the pressure of light, using a resonance method. Though Saha had little real skill or aptitude for practical work, he always took the keenest interest in and gave every encouragement to experimental work going on in his laboratory, and often suggested fruitful problems for investigation. He held that in a progressive department of physics, research should be vigorously pursued in both theory and experiment, and a high place given to serious and good teaching. Saha's first work in astrophysics, and it was a very important one, was the formulation of the concept of *selective radiation pressure*, and the recognition of its role in the relative distribution of the elements in the solar atmosphere. (He had read in Agnes Clarke's book about the 'hypothetical levitative force' which apparently acted on atoms of some elements only, e.g. calcium.) A short note 'On radiation pressure and the quantum theory' appeared in the *Astrophysical Journal* in 1919. A detailed paper communicated to the same journal was for some reasons—partly the length of the paper—not accepted for publication, and it was published in the relatively little-known *Journal of the Department of Science, Calcutta University*. Saha did not pursue the subject of selective radiation pressure further. It was raised to an entirely new level by E. A. Milne. In 1918 on the basis of his work in the field of electromagnetic theory and radiation pressure, Saha was awarded the D.Sc. degree of the Calcutta University. His examiners were O. W. Richardson, N. R. Campbell and Porter.

Saha's greatest contribution is, undoubtedly, the theory of high-temperature ionization and its application to stellar atmospheres. The equation that goes by his name was first given in the paper 'On ionization in the solar chromosphere', published in the *Philosophical Magazine* for October 1920. Using the language of physical chemistry he called it the 'equation of the reaction-isobar for ionization'. Discussing the case of the ionization of calcium, he wrote:

'We may regard the ionization of calcium atom as taking place according to the following scheme, familiar in physical chemistry,

$$\mathrm{Ca} \rightleftharpoons \mathrm{Ca}_{+} + e - U$$

where Ca is the normal atom of calcium (in the vapour state), Ca_{+} is an atom which has lost one electron, U is the quantity of energy liberated in the process. The quantity considered, is 1 g atom . . . To calculate the "Reaction-isobar" K, let us assume that P is the total pressure, and a fraction x of the Ca-atom is ionized.

Then we have

$$\log K = \log \frac{x^2}{1-x^2} P = -\frac{U}{4\cdot571\ T} + 2\cdot5 \log T - 6\cdot5$$

This is the equation of the "reaction-isobar" which is throughout employed for calculating the "electron-affinity" of the ionized atom.' The value 6·5 in the above expression is the value of the chemical constant obtained from the Sackur-Tetrode-Stern relation.

It is pertinent to remark that the ionization theory was formulated by Saha, working by himself in Calcutta, and the paper quoted above was communicated by him from Calcutta to the *Philosophical Magazine*—incorrectly, statements to the contrary have sometimes been made. (Saha's first visit to Europe was made a couple of months later.) Further papers soon followed. It is not too much to say that the theory of thermal ionization introduced a new epoch in astrophysics by providing for the first time, on the basis of simple thermodynamic considerations and elementary concepts of the quantum theory, a straightforward interpretation of the different classes of stellar spectra in terms of the physical conditions (temperature and to a lesser extent pressure) prevailing in the stellar atmospheres. S. Rosseland in the Introduction to his well-known *Theoretical astrophysics* (Oxford University Press, 1936) has observed: 'Although Bohr must thus be considered the pioneer in the field, it was the Indian physicist Meghnad Saha who (1920) first attempted to develop a consistent theory of the spectral sequence of the stars from the point of view of atomic theory. Saha's work is in fact the theoretical formulation of Lockyer's view along modern lines, and from that time the idea that the spectral sequence indicates a progressive transmutation of the elements has been definitely abandoned. From that time dates the hope that a thorough analysis of stellar spectra will afford complete information about the state of the stellar atmospheres, not only as regards the chemical composition, but also as regards the temperature and various deviations from a state of thermal equilibrium, the density distribution of the various elements, the value of gravity in the atmosphere and its state of motion. The impetus given to astrophysics by Saha's work can scarcely be overestimated, as nearly all later progress in this field has been influenced by it and much of the subsequent work has the character of refinements of Saha's ideas.'

It may be of some interest to relate how Saha was led to the ionization theory. In his own words*: 'It was while pondering over the problems of astrophysics, and teaching thermodynamics and spectroscopy to the M.Sc. classes that the theory of thermal ionization took a definite shape in my mind in 1919. I was a regular reader of German journals, which had just started coming after four years of First World War, and in course of these studies, I came across a paper by J. Eggert in the *Physikalische Zeitschrift* (p. 573), Dec. 1919, "Über den Dissoziationzustand der Fixsterngase" in which he applied Nernst's Heat Theorem to explain the high ionization in stars due to high temperatures, postulated by Eddington in course of his studies on stellar structures.†

'Eggert, who was a pupil of Nernst and was at the time his assistant, had given a formula for thermal ionization, but it is rather strange that he missed the significance of ionization potential of atoms, importance of which was apparent from the theoretical work of Bohr, and practical work of Franck and Hertz which was attracting a good deal of attention in those days. . . . Eggert used Sackur's formula of the chemical constant for calculating that of the electron, but in trying to account for multiple ionization of iron atoms in the interior of stars on this basis, he used very artificial values of ionization potential.

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* Extract (taken from a copy in the possession of Dr A. K. Saha, son of Professor Saha) from a letter dated 18 December 1946, to Professor H. H. Plaskett, University Observatory, Oxford.

† Saha in his paper 'On ionization in the solar chromosphere' makes a handsome acknowledgment to Eggert's work.

'While reading Eggert's paper I saw at once the importance of introducing the value of "ionization potential" in the formula of Eggert, for calculating accurately the ionization, single or multiple, of any particular element under any combination of temperature and pressure.

'I thus arrived at the formula which now goes by my name. Owing to my previous acquaintance with chromospheric and stellar problems, I could at once see its application. I prepared in the course of six months of 1919 (February to September) four papers and communicated them for publication in the *Philosophical Magazine* from India within August to September.'

One of the four papers above referred to was 'On the Harvard classification of stellar spectra'. When shortly after communicating the paper Saha went to London, he withdrew it from the *Philosophical Magazine*, and completely rewrote it in Professor A. Fowler's spectroscopy laboratory in the Imperial College of Science and Technology, London. The revised (and, no doubt, considerably expanded and improved) paper was published in the *Proceedings of the Royal Society* (1921) under the title, suggested by Fowler, 'On a physical theory of stellar spectra'. Saha had the greatest regard for Professor Fowler, and always spoke with warmth and gratefulness of the encouragement and help he had received from him in London. Years afterwards he wrote:* 'I took about four months in rewriting this paper, and all the time I had the advantage of Professor Fowler's criticism, and access to his unrivalled stock of knowledge of spectroscopy and astrophysics. Though the main ideas and working of the paper remained unchanged, the substance matter was greatly improved on account of Fowler's kindness in placing at my disposal fresh data, and offering criticism wherever I went a little astray, out of mere enthusiasm.'† This paper and the one on the 'Ionization of the solar chromosphere' are by far the most significant and original of Saha's many scientific contributions.

As one of the interesting and immediate results following from the ionization theory, Saha not only was able to explain the absence in the solar spectrum of the lines of Rb and Cs because of the low ionization potential of these elements, but also to predict that their resonance lines were likely to be observed in the relatively cooler regions of the sunspots. H. N. Russell, following Saha's prediction, looked for and found the infra-red pair of Rb-lines, but no Cs-lines, in the Mount Wilson photographs of the spot spectra which, incidentally, had been taken before the publication of Saha's theory.

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* Letter to Professor H. H. Plaskett referred to in the preceding footnote.

† Professor H. Dingle once observed: 'On thinking back to the relation which existed between Saha and Fowler I am tempted to compare it with that between Maxwell and Faraday'. (*The Observatory,* **66,** 22 (1945).

We may recall at this place that, apart from J. Eggert's paper referred to earlier, a definite suggestion about high-temperature ionization, prior to Saha's papers, was also made by the Oxford physicist F. A. Lindemann (later Lord Cherwell) in connexion with his controversy with S. Chapman about the origin of magnetic storms (*Philosophical Magazine*, December 1919). He gave the ionization formula for hydrogen, and discussed the possibility of the complete ionization of hydrogen in the solar chromosphere. Lindemann, however, did not further develop or generalize the formula; and above all he failed to notice that the theory of thermal ionization (with accurate values of ionization potentials obtained from spectroscopic data or otherwise) constitutes the key to the interpretation of stellar spectra with their almost bewildering complexity. It was Saha who first recognized this, and worked out the consequences in considerable detail, and he did this independently of Lindemann's suggestion about the high-temperature ionization of hydrogen.

In 1919 Saha was awarded the Premchand Roychand Scholarship of the Calcutta University, and this made it possible for him to spend some two years in Europe. He first went to London and spent about five months in the laboratory of Professor A. Fowler. Later he moved to W. Nernst's laboratory in Berlin, and did some experimental work on the conductivity of heated caesium vapour to seek an experimental verification of the theory of thermal ionization. The results were inconclusive. Some years later, when at Allahabad, Saha again returned to this problem, and an account of the investigation, jointly with K. Majumdar and N. K. Sur, was published in the *Zeitschrift für Physik* (1926).

In the early twenties R. H. Fowler (in collaboration with C. G. Darwin) developed a very powerful method in statistical mechanics permitting a systematic exposition and working out of the equilibrium properties of matter. He used this to provide a (rigorous) derivation of the ionization formula which as described earlier Saha had obtained by extending (and justifiably) to ionization of atoms the theorem of van't Hoff, well known in physical chemistry for its application to molecular dissociation. Also, a significant improvement in the Saha equation introduced by Fowler was to include the effect of the excited states of atoms and ions. Further, it marked an important step forward when in 1923 E. A. Milne and R. H. Fowler in a paper in the *Monthly Notices of the Royal Astronomical Society* showed that the criterion of the *maximum intensity* of absorption lines (belonging to subordinate series of a neutral atom) was much more fruitful in giving information about physical parameters of stellar atmospheres than the criterion employed by Saha which consisted in the *marginal appearance or disappearance* of absorption lines. (The latter criterion requires some knowledge of the relevant pressures in the stellar atmospheres, and Saha following the generally accepted view at the time assumed a value of the order of 1 to 0·1 atmosphere.) To quote from E. A. Milne:* 'Saha had concentrated on the marginal appearances and disappearances of absorption lines in the stellar sequence, assuming an order of magnitude for the pressure in a stellar atmosphere and calculating the temperature where increasing ionization, for example, inhibited further absorption of the line in question owing to the loss of the series electron. As Fowler and I were one day stamping round my rooms in Trinity and discussing this, it suddenly occurred to me that the *maximum* intensity of the Balmer lines of hydrogen, for example, was readily explained by the consideration that at the lower temperatures there were too few excited atoms to give appreciable absorption, whilst at the higher temperatures there are too few neutral atoms left to give any absorption . . . That evening I did a hasty order of magnitude calculation of the effect and found that to agree with a temperature of 10000° for the stars of type AO, where the Balmer lines have their maximum, a pressure of the order of 10^{-4} atmosphere was required. This was very exciting, because standard determinations of pressures in stellar atmospheres from line shifts and line widths had been supposed to indicate a pressure of the order of one atmosphere or more, and I had begun on other grounds to disbelieve this.'

In November 1921 Saha returned from Europe and joined the University of Calcutta as Khaira Professor of Physics, a new chair created from the endowment of Kumar Gurprasad Singh of Khaira. In 1923 Saha left Calcutta to take up the appointment of Professor and Head of the Physics Department in the University of Allahabad. He held this appointment for 15 years. At Allahabad he gave a large part of his time to teaching—he regularly lectured to undergraduate and postgraduate classes. His lectures were systematically and carefully prepared, and a very large part of what he spoke, he wrote on the blackboard in his characteristically clear and bold handwriting. He made copious use of lantern slides, and was fond of demonstration experiments.†

He initiated and organized research in several subjects, such as statistical mechanics, atomic and molecular spectroscopy, experiments on thermal ionization and 'electron affinity' of electronegative elements, active modification of nitrogen, high-temperature dissociation of molecules, and ionospheric propagation of radio waves and physics of the upper atmosphere. In 1926 Saha presided at the Physics and Mathematics Section of the Indian Science Congress Association. His address was on thermal ionization. Within less than a decade of Saha's arrival the Physics Department of the Allahabad University became one of the most active centres of research in the country, particularly in the field of spectroscopy. Amongst his earliest associates in research mention may be made of P. K. Kichlu (now Professor of Physics in the Delhi University), K. Majumdar and N. K. Sur. The Department attracted students from all over India. R. C. Majumdar, now Professor of Physics in the Delhi University, S. Basu, the present Director-General of the Department of Meteorology, and D. S. Kothari were Saha's students at Allahabad. It was in 1927 that at the age of thirty-four Saha was elected to the Royal Society. The United Provinces Government sanctioned a personal annual grant of Rs. 5000 for his research work.

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* E. A. Milne, Obituary Notice of R. H. Fowler (1889-1944), *Obituary Notices of the Fellows of the Royal Society*, **5**, 61-78 (1945).

† Saha often quoted the following rather remarkable passage from the 9th century Sanskrit work, *Rasendra-Chintamani*:

'I have heard much from the lips of savants, I have seen many (formulae) well established in Scriptures, but I am not recording any which I have not done myself.

'I am only recording those fearlessly which I have carried out before my elders with my own hand. They are alone to be regarded as real teachers who can show by experiments what they teach. They are the deserving pupils, who, having learnt from their teachers can actually perform them and improve upon them. The rest are merely stage-actors.'

In 1927, at the invitation of the Italian Government, Saha attended the Volta Centenary Celebration held at Como, and presented a paper 'On the explanation of complicated spectra of elements'. He later joined a total solar eclipse expedition to Ringebu (68° N) led by L. Vegard of Oslo University. In 1936 he was elected to an overseas fellowship of the Carnegie Trust of the British Empire, and he visited Germany, England, and the United States and spent about two months with H. Shapley at the Harvard College Observatory. (Saha took keen interest in archaeology and ancient history, and on his way to Europe he visited the ruins of Ur of the Chaldees then recently excavated by Sir Leonard Woolley.) In a paper 'On a stratospheric astrophysical observatory' which appeared in the *Records of Harvard College Observatory*, Saha made what at the time (1936) was a very ambitious plea of photographing the solar spectrum at a height of some 50 kilometres, well above the ozone layer; and he pointed out the enormous gain that would accrue for astrophysics. In this paper, as also in that on the action of ultra-violet sunlight upon the upper atmosphere published in the *Proceedings of the Royal Society* (1937) and in his Presidential Address to the National Institute of Sciences of India given at Lahore in 1938, he discussed the possibility that the ultra-violet radiation from the sun may be several orders of magnitude above that corresponding to a black-body at about 6500 °K. He wrote: 'This may possibly be due to the fact that the ultra-violet spectrum of the sun may consist of a continuous background of faint light on which are superposed emission lines of H, He, He^+, Fe^+, and other elements which are represented in the visible range by lines of subordinate series, or by patches of ultra-violet continuous light (near about $\lambda 500$) leaking through the solar atmosphere from a much hotter region inside the photosphere, as suggested by Professor H. N. Russell.'*

In December 1937, Sir Arthur Eddington visited India as a member of the delegation of the British Association for the Advancement of Science to the Jubilee Session (1938) of the Indian Science Congress Association held in Calcutta. (Lord Rutherford was to preside at the session but he died a few months before, and his place was taken by Sir James Jeans.) At Saha's invitation Eddington paid a short but memorable visit to Allahabad; he was presented a civic address by the Allahabad Municipality.

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* M N. Saha. *Proc. Roy. Soc.* A, **160**, 55 (1937).

In 1938 Saha was offered the Palit Chair in Physics at the University of Calcutta, vacated some years earlier by C. V. Raman who had gone to Bangalore as Director of the Indian Institute of Science. (Raman was succeeded by D. M. Bose who was Palit Professor from 1932 to 1937.) Saha accepted the offer and left Allahabad after a stay of fifteen years there. He occupied the Palit Chair for fifteen years, retiring in 1953 at the age of 60. In Calcutta Saha could not give as much of his time to his research as was possible in the relatively quiet atmosphere of Allahabad. Here a large part of his time was taken up in the administration of the laboratories, in building a new Institute of Nuclear Physics, and the reorganization and expansion of the laboratories of the Indian Association for the Cultivation of Science. Apart from all this, after the partition of India in 1947, he gave a substantial part of his time and energy to the massive human and economic problem of the 'refugees' from East Bengal (Pakistan)—it was a call that a man, himself from East Bengal, of his temperament and intense sensibility could not resist.

His researches in Calcutta were concerned largely with the systematics of atomic nuclei, particularly beta-activity; the propagation of electromagnetic waves in the ionosphere; and the problem of the solar corona. In the case of the corona an outstanding problem is that of the mechanism or source responsible for the high-degree ionization—loss of 9 to 13 electrons—of the iron atoms, and also nickel in the inner corona, as conclusively demonstrated by B. Edlen's remarkable work (1938) on the origin of the coronal bright spectral lines. D. Kundu working under Saha showed that some of the lines may be due to highly ionized cobalt atoms. The intense ionization, as also the excessive broadening of the Fraunhofer lines in the scattered radiation from the outer corona, and the strong coronal radio-emission in the region of wavelengths of the order of a few metres, all suggest coronal temperatures of the order of millions of degrees, but the origin of these temperatures which are comparable to those occurring in stellar interiors is largely still an unsolved problem. Saha found it difficult to accept the existence of such high temperatures. In fact, to account for the Edlen lines he advanced in a number of papers the somewhat quaint hypothesis that the highly charged ions necessary for the emission are produced as a result of some form of nuclear fission occurring in the sun's outer atmosphere—he suggested tri- or quadri-fission of U or Th. The problem of the emission of radio waves from the sun and other stellar bodies also engaged his attention, and he discussed the likely role of the magnetic field and the hyperfine-structure level-splitting of the ground state of the H-atom (*Nature, Lond.* **158,** 717 (1946)). He failed to recognize, at any rate explicitly, the possibility of the occurrence as *line emission* of the 21-cm hydrogen line in radio spectra. (He was not aware at the time of the earlier prediction (1944) of H. C. van de Hulst.) In collaboration with B. D. Nag Chaudhuri (the present Palit Professor of Physics) Saha investigated the problem of the geological age of some of the Indian rocks.

Saha early realized the growing importance of nuclear physics and the impact it was likely to make on the country's scientific and industrial progress. He felt that in the context of current developments there was a real and urgent need for a separate institution of nuclear physics devoted to post-graduate study and research. The result of his dedicated and untiring efforts was the Institute of Nuclear Physics in Calcutta, founded in April 1948 (and formally opened in 1950). The Institute is attached to the University. It runs a post-graduate course, and its research activities cover, amongst other subjects, beta-ray spectroscopy, nuclear resonance, and the use of radioactive isotopes in medicine. There is also an active theoretical group; a separate biophysics section and an instrument section. It has a 38-inch cyclotron—the magnet and some of the other critical parts were obtained from Lawrence's Laboratory in Berkeley. Saha was elected Honorary Director of the Institute for life. After his death, the Institute befittingly has been named after him.

Though the Institute of Nuclear Physics took much of Saha's energy and time, yet there was another institution, the Indian Association for the Cultivation of Science, which was always in his mind, and for its reorganization and expansion he worked hard and ceaselessly. The Association, the oldest institution devoted to science and its cultivation in modern India, was founded in 1876 by a private medical practitioner of Calcutta, Dr Mahendra Lal Sircar. He intended it 'to combine the character, scope and objects of the Royal Institution of London and of the British Association for the Advancement of Science'. As is well known the Raman effect was discovered in the laboratories of the Association in 1928. At the time Raman was Palit Professor of the University and also the honorary head of the Laboratories of the Association.

Saha, shortly after his coming to Calcutta as Palit Professor in 1938 (Raman left Calcutta in 1932) began to take a keen interest in the activities of the Association. He was elected the Honorary Secretary in 1944, and in 1946 he was elected the President of the Association. He was succeeded by J. C. Ghosh in 1950. The large-scale expansion in the research activities of the Association which has taken place in recent years is almost entirely the result of Saha's initiative, resourcefulness and devoted work. The Association has now a new building in Jadavpur (Calcutta) where it moved in 1951 from 210 Bowbazar Street.

In 1952 the post of a full-time Director of the Laboratories of the Association was created and the choice of the first Director obviously fell on Saha. He accepted the appointment in 1953 on retirement from the Palit Chair of the University which he held for fifteen years. He was Director of the Laboratories of the Association and Honorary Director of the Institute of Nuclear Physics at the time of his death in 1956.

Saha was the General President of the twenty-first annual session of the Indian Science Congress Association held in Bombay in January 1934. The first part of his address dealt with some of the current astrophysical problems, and in the second part he advocated the formation of an All-India Academy of Sciences. In the same address he drew pointed attention to the serious problem—still largely unsolved—of recurring disastrous floods in many Indian rivers, and stressed the need for a River Research Laboratory to make a scientific study of the complex problems involved in flood control and river utilization generally. He was one of the first in India to realize the great importance of this subject. He had personal experience of some of the catastrophic floods in the Damodar Valley area (Bengal), and he often took an active part in relief measures organized to deal with them. The writings of Saha, many of which appeared in *Science and Culture*, on flood control and harnessing the water of the rivers were to no small extent responsible in creating a public awareness of these problems. In 1943 the Bengal Government appointed the Damodar Flood Enquiry Committee. Saha was a member of the Committee, and his contribution was widely recognized as a vital one. Largely as a result of the committee's report came, eventually, the multi-purpose Damodar Valley Project (very much on the lines of the Tennessee Valley Authority in the U.S.A.) which has been the forerunner of the Bhakra-Nangal and other multi-purpose river projects in India. The establishment of the River Research Institute (Haringhata, Calcutta) was also, to no small extent, the result of Saha's initiative and efforts.

It was during the early thirties that the National Academy of Sciences at Allahabad (originally called the Academy of Sciences for the Provinces of Agra and Oudh), the Indian Academy of Sciences at Bangalore, and the National Institute of Sciences at Calcutta (headquarters transferred to Delhi in 1946) were founded. In the case of the Allahabad Academy, and so also the National Institute, the role of Saha was of the utmost importance. He was the first President (1932-34) of the Allahabad Academy and the second President (1937-39) of the National Institute of Sciences. (The first President of the Institute was Sir Lewis Fermor, the Director-General of the Geological Survey of India.) Saha was President of the Royal Asiatic Society of Bengal (now the Asiatic Society) for 1944-46.

Saha from the very beginning was closely associated with the work of the Indian Council of Scientific and Industrial Research (which corresponds to the D.S.I.R. in the U.K.) first constituted in 1942. He was a member of the Governing Body; and was for many years chairman of the Atmospheric Research Committee, and chairman or member of several other research and planning committees of the C.S.I.R. He was closely connected with the planning and establishment of the Central Glass and Ceramics Research Institute at Calcutta—one of the national laboratories under the C.S.I.R.—and was for many years chairman of its Advisory Committee. He was the chairman of the Indian Calendar Reform Committee, appointed by the C.S.I.R. in 1953.

Saha was a member of the University Education Commission appointed by the Government of India in 1948 under the chairmanship of Dr S. Radhakrishnan.

The monthly journal *Science and Culture*, published by the Indian Science News Association (Calcutta), was started by Saha in 1934. The journal is now running in its 24th volume. Saha wrote prolifically for the journal, and his many articles and editorials bear witness to his remarkable versatility, and his passionate interest in and grasp of the many scientific, industrial and economic problems facing the country. A list of his contributions is included in the bibliography at the end.

Saha was a keen student and ardent advocate of social and economic planning in India, particularly, in its relation to science, large-scale industry and technology. His general philosophy in these matters is perhaps best expressed in the following words (*Nature, Lond.* **155**, 221 (1945)): 'The philosophy of kindliness and service to our fellow-men was preached by all founders of great religions, and no doubt some great kings and ministers of religions in every country and at all ages tried to give effect to this (altruistic) philosophy. But the efforts were not successful, for the simple reason that the methods of production of commodities were too inefficient to yield plenty for all, which is an indispensable condition for practical altruism. We can, therefore, hold that *so far as individual life is concerned*, science has achieved a target aimed at by the great founders of religions in advanced countries of the world. The effects of maldistribution of wealth, due to historical causes, are being rapidly cured by the introduction of social laws.'

He was an active member of the National Planning Committee appointed by the Indian National Congress in 1938. Saha was chairman of the power and fuels sub-committee, and member of the sub-committee on river training and irrigation.

In 1951 Saha was elected, as an independent candidate, to the Indian Parliament from the North-West Calcutta constituency. His contribution to the debates in the House, because of his special knowledge and background, was particularly valuable in relation to subjects such as education, industrial policy, river-valley projects and atomic energy.

Saha was widely travelled in Europe and the U.S.A. He had been twice to the U.S.S.R. He was the Indian delegate to the 220th Anniversary Celebrations (1945) of the Soviet Academy of Sciences.

In March 1954 the 60th birthday of Saha was celebrated in Calcutta, Allahabad and Delhi by his many friends, old students and admirers. A Commemoration Volume *Professor Meghnad Saha—His life, work and philosophy*, edited by S. N. Sen was published on the occasion. (The photograph of Professor Saha facing page 217 was taken in his 60th year.)

During the last years of his life Saha was keeping an indifferent health. He was suffering from high blood pressure. But the end came rather suddenly. He passed away on 16 February 1956.

The life of Saha was in a sense an integral part of the growth of scientific research and progress in India, and the effect of his views and powerful personality would be felt for a long time to come in almost every aspect of scientific activity in the country. His dedication to science, his forthrightness and utter disregard of personal comforts in the pursuit of his chosen vocation will long remain an inspiration and an example.

Saha was married in June 1918 to Shrimati Radha Rani Saha. Her kindliness and genuine simplicity of character have won for her the affection and respect of generations of students and colleagues of Saha. Saha is survived by his wife, three sons and three daughters. The eldest son, A. K. Saha, is Professor in the Institute of Nuclear Physics.

In the preparation of this memoir I have drawn rather freely on the Commemoration Volume presented to Saha on his 60th birthday.

D. S. KOTHARI

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JOHN LIONEL SIMONSEN

1884-1957

John Lionel Simonsen was born in Levenshulme, Manchester, on 22 July 1884. His father, Lionel Michael Simonsen, was a merchant of Danish origin, who became a naturalized British subject. His mother, Anna Sophie, daughter of J. M. Bing of Copenhagen, had academic relations. Her brother was Professor of Medicine in the University of Copenhagen, and her uncle was Professor Carl Warburg, Professor of Literature in Gothenburg and later in Stockholm. Simonsen had one sister, his senior by two and a half years; it was generally supposed that one or both of his parents were of Jewish race.

He married on 30 December 1913, Jannet Dick Hendrie (M.B., Ch.B., Edinb.), daughter of Robert Hendrie of Nairn. Lady Simonsen was at the time Superintendent of the Caste and Gosha Hospital, Madras. She was an exceptionally brilliant surgeon and during the first World War acted as Professor of Pathology at the Lady Hardinge Medical College, Delhi and later as head of the Malaria Research Station in the Simla Hills. The late Sir Harry Lindsay was a close friend of the Simonsens and wrote as follows:

> 'I first met John Simonsen and his wife Jannet at Dehra Dun in 1920; our friendship was founded upon a joint interest in the future of the Indian lac industry and was cemented when Mrs Simonsen's (as she then was) medical skill diagnosed an attack of influenza-cum-pneumonia, from which I was suffering when I joined them—a poor introduction to their hospitable house! However, they and the civil surgeon saw me through and it is no exaggeration to say that I owe my life to their skill and care.'

It may perhaps be anticipated and interpolated here that Simonsen had many later contacts and joint interests with Sir Harry Lindsay; Indian scientific affairs, Colonial Products, The Royal Society of Arts, of which he became a Fellow in 1943 with service on the Council from 1949 until his death.

Simonsen's early school days were spent at Miss Herford's Ladybarn House School and at a private school at Rusholme, Manchester.

From 1898-1901 he went to Manchester Grammar School where he came under the influence of a renowned teacher of chemistry, Francis Jones. He was not alone in paying tributes to the stimulus received from this remarkable man. Contemporaries at the School were C. S. Gibson (later F.R.S.), D. M. S. Watson (later F.R.S.), and K. Fisher who was later Headmaster of Oundle.

During the summer holidays, Simonsen worked in Copenhagen at the

Landbrughy Skole where his uncle by marriage, Professor V. Henriques, was head of the Physiology Department. This experience turned his interest in the direction of science and he entered (1901) the University of Manchester as a student in the Honour School of Chemistry, thus forfeiting a Scholarship awarded to him with Modern Languages in view. He graduated in 1904 with 1st Class Honours and became D.Sc. in 1909. The standard for this Doctorate at Manchester was (and remains) a high one and the interval of five years was short. He was the first Schunck Research Fellow in Chemistry in the University and was succeeded in this appointment by Arthur Lapworth. Simonsen's own account of the Chemistry School at Manchester is worthy of citation.

'The head of the department of chemistry was Professor H. B. Dixon, Professor W. H. Perkin being Professor of organic chemistry. Other members of the chemistry staff with whom I came into close contact were W. A. Bone, J. F. Thorpe, G. H. Bailey and D. L. Chapman. Bone was the head of the elementary laboratory in which I first worked and in my opinion there is no doubt that the lack of "spoon feeding" was of the greatest possible value in developing initiative and originality. For example, in my second term I was told to prepare nickel carbonyl from a nickel ore. No assistance was given and I was simply left to it. As the subject was quite new it meant looking up the original papers but it was a month well spent. Bailey's instruction in quantitative methods was sound if a little antiquated, whilst Thorpe made dyestuff chemistry very attractive. Chapman made some attempt to teach physical chemistry but this was a much neglected side. Dixon's lectures to first year honours students were brilliant both in form, delivery and experiments. Perkin's lectures on organic chemistry were extremely clear and have been frequently praised, they made organic chemistry appear very simple.'

The chemistry school at Manchester in Simonsen's days was an active centre of research both in inorganic (physical) and organic chemistry. On the former side the names of H. B. Dixon, W. A. Bone, D. L. Chapman, R. V. Wheeler, and H. F. Coward come first to mind, whilst on the latter associated with W. H. Perkin, Jun., were *inter alia*, S. Pickles, J. F. Thorpe, F. W. Kay, Chaim Weizmann, R. Robinson, W. N. Haworth, V. J. Harding, V. Tattersall and P. Engels. Arthur Lapworth came just a little later; F. L. Pyman was in Simonsen's year but went to Zürich to work with Bamberger. Schoolfellow K. Fisher went from the M.G.S. to Jena, where he studied under Knorr, but he later joined us in Perkin's laboratory as a research student and occupied himself with that fickle jade 'sylvestrene', or so he was wont to say. His exuberant temperament provided a tonic at all times and greatly improved our amenities. Another denizen of the laboratory, a year or so later than Simonsen, was C. J. T. Cronshaw, also a great wit and destined to become a leader in the organic chemical industry.

In this galaxy 'Johnnie' was by common consent the star experimentalist. It was recognized that his way with a 'Perkin Triangle' (a device for collecting fractions during distillation under reduced pressure) reached the highest possible level of craftsmanship and that if he could not crystallize a substance, it would be a waste of time for anyone else to try. His perfection of bench technique was probably an outcome of a characteristic loyalty, in this case to the scientific tradition fostered by his teachers, and of his sense of the essential rightness of conformity to the principles inculcated by those whom he accepted as guides.

We were at one time both concerned with the activities of a junior collaborator and went to his bench to see the determination of a crucial mixed melting point. Mr X mixed the specimens with too much delicacy; I saw John smile and when X's back was turned he said to me 'mix it properly'. Doubtless a trivial incident but somehow remembered over a long period of years.

Between 1907 and 1909, Simonsen was an Assistant Lecturer and Demonstrator at the University of Manchester. His research efforts were largely directed towards the synthesis of norpinic acid and though this objective was not reached, he carried out numerous syntheses using the classical reactions and made a number of intermediates later found useful by other chemists. An elegant application of the Grignard reaction led to syntheses of terebic, terpenylic, and homoterpenylic acids and to his first independent publication.

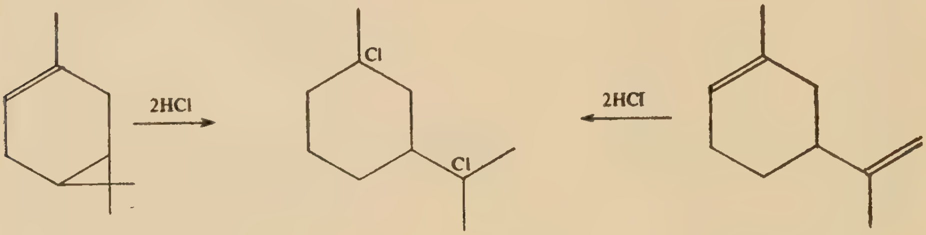
We collaborated in a study of barbaloin which resulted in a novel view of the nature of aloemodin and rhein. At that time the latter substance, obtained from natural sources, was thought to be the methylene ether of a tetrahydroxyanthraquinone. The oxidation of acetylated barbaloin with chromic anhydride in acetic acid+acetic anhydride solution furnished rhein diacetate in reasonably good yield. Reinvestigation of rhein showed that it is dihydroxyanthraquinone carboxylic acid. Furthermore it was shown that the relationship of chrysophanic acid, aloemodin, and rhein are in accordance with the formulae $R\cdot CH_3$, $R\cdot CH_2OH$, $R\cdot CO_2H$ respectively (R=dihydroxyanthraquinonyl). Rhein dimethyl ether was degraded by the Hofmann reaction, applied to its amide, to aminodimethoxyanthraquinone and thence to dimethoxyanthraquinone. This was not obtained quite pure and was by exclusion of alternatives considered to be derived from *iso*chrysazin. Later the orientation of hydroxyl groups was shown by Oesterle to be that of chrysazin. Many years later Simonsen resumed the study of barbaloin and by careful analyses established facts which facilitated the eventual solution of the problem.

In 1910, when twenty-six years of age, he became Professor at the Presidency College, Madras. There he remained until 1919 and not only continued his scientific work (partly in collaboration with C. S. Gibson, then at Travancore) but also took a keen interest in Indian scientific work and organizations in general.

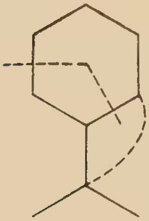
In 1914 he was a founder member of the Indian Science Congress Association, which owed much to his personal initiative. He acted as General

Secretary at the start and continued in this office until 1926. During the first World War he was Controller of Oils and Chemical Adviser to the Indian Munitions Board.

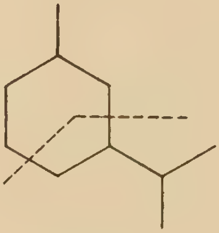
From 1919-1925 he became Chief Chemist of the Forest Research Institute and College at Dehra Dun. It was here that he made the very interesting discovery that Indian turpentine consisted largely of Δ^2-carene, replacing the isomeric pinenes of other turpentine oils. This was shown to be the source of sylvestrene dihydrochloride already known to be produced by the action of hydrogen chloride on the oil.



Δ^2-carene sylvestrene



usual 'head to tail' arrangement



two isopentanes but not 'head to tail'

Whilst sylvestrene can be dissected into two *iso*pentane units, the mode of union of these infringed a secondary 'head to tail' principle. The recognition of the origin of sylvestrene dihydrochloride from carene reinstated the secondary as well as the primary rule in this case at least.

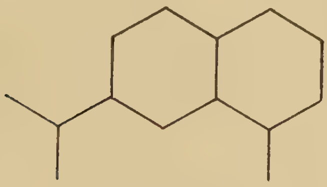
The molecular structure of Δ^2-carene followed a study of its transformations but some details, known by Simonsen as difficult of explanation, have only quite recently been cleared up (Widmark 1957).

In 1925, Simonsen became Professor of Organic Chemistry at the Indian Institute of Science, Bangalore. Here he continued studies of Indian essential oils and, as ever, was concerned with administrative and general educational problems in the scientific and especially chemical field in India. For example in 1919-1920 he was a Member of the Indian Chemical Services Committee of which Sir Jocelyn Thorpe was Chairman and jointly with whom he wrote the *Report*. At about this period he received the Gazetted thanks of the Indian

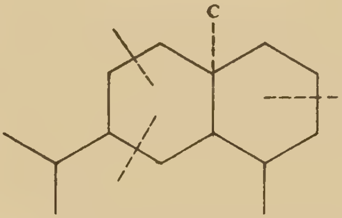
Government for services on the Indian Munitions Board. The Chairman was Sir Thomas Holland who made Simonsen his Chemical Adviser. It should be recorded that an eloquent tribute to Holland's ability and integrity is included in Simonsen's autobiographical notes which have been consulted for the present purpose.

After returning to England in 1928, he worked for some time in C. S. Gibson's chemical department at Guy's Hospital until in 1930 he was appointed Professor of Chemistry in the University of Wales at University College, Bangor. Here he collaborated with A. E. Bradfield and also, most fruitfully, with A. R. Penfold of the Technological Museum, Sydney, N.S.W. Penfold isolated many new terpenoids from Australian sources and many of these were studied at Bangor.

Among the plants studied by Penfold and Simonsen and their collaborators was the small tree *Eremophila mitchelli*, known as bastard sandalwood or budtha, widely distributed in some of the drier parts of Australia. The wood was found to contain a sesquiterpene ketone, $C_{15}H_{22}O$, termed eremophilone, as well as a hydroxy and hydroxy-dihydro-derivative. Eremophilone was first converted into dihydroeremophilol by reduction with sodium and alcohol and this tetra-hydro derivative furnished eudalene (1-methyl-7-*iso*propylnaphthalene) on dehydrogenation by heating with selenium. A carbon atom must be added to the eudalene skeleton (C_{14}), evidently in one of the angle positions, and to comply with the isoprene rule Simonsen naturally assumed the structure shown below.

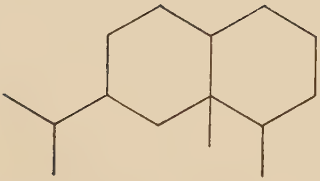


eudalene skeleton

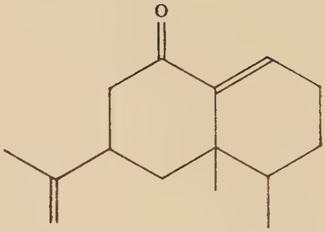


tri-*iso*pentane skeleton

Catalytic reduction of eremophilone gives a second tetrahydro-derivative which is a ketone and, to mark the position of the carbonyl, this substance was changed to a *t*-alcohol by the Grignard reaction using methylmagnesium iodide. Selenium dehydrogenation then afforded 1:5-dimethyl-7-*iso*propylnaphthalene. Hence the carbonyl group must be in asterisked position in the above C_{15} structure. Other evidence showed the presence of the isopropenyl group ($-CMe{=}CH_2$) and the *αβ*-unsaturated ketone group ($CO{\cdot}C{=}C$). It was found impossible to accommodate all the known facts on the basis of the tri-*iso*pentane skeleton and hence it was thought that a methyl migration must have occurred in the biosynthesis of the ketone so as to give the skeleton below, and when this was assumed the molecular structure of eremophilone could be acceptably formulated.

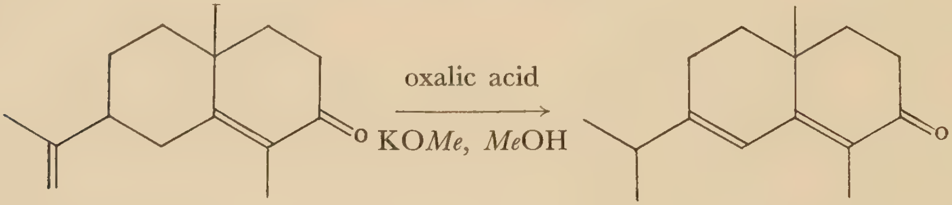


skeleton after methyl migration



eremophilone

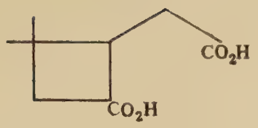
Confirmatory evidence of the correctness of this view was soon forthcoming (e.g. degradation to *o*-xylene by way of 1 : 2-dimethylcyclohexylacetic acid, which was itself synthesized). The full configuration of hydroxydihydroeremophilone has been established by X-ray crystallography (Grant & Rogers); it turns out to be a derivative of *cis*-decalin. This investigation was probably the first in which biosynthetic methyl-migration was recognized; the phenomenon became later of great importance in many series, especially the steroids. On the other hand α-cyperone, $C_{15}H_{22}O$, first isolated from the tubers of *Cyperus rotundus* by Hegde & Rao, has been shown by Simonsen and his colleagues (Bradfield, Gillan, Rao, Hegde & Pritchard) to be based on the normal eudalene structure with the angle methyl in what was regarded above as the normal position (methyl *not* migrated).



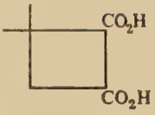
α-cyperone β-cyperone

The writer, with du Feu and McQuillin, developed a synthesis of cyclohexanones which was seen to be peculiarly well suited for the synthesis of α-cyperone. Hence a welcome collaboration was undertaken and ketones structurally identical with cyperone were obtained (Adamson, McQuillin, Robinson & Simonsen, 1937). Later, Roy (1954) applying the same general method has synthesized β-cyperone, the position of the migrated double bond having been suggested by McQuillin (1951, 1955, 1956).

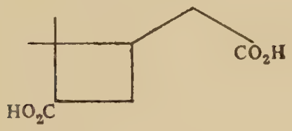
The very difficult problem presented by the sesquiterpene caryophyllene was attacked by Simonsen in collaboration with G. R. Ramage and the significant discovery made of the cyclobutane ring in the molecule. The isolation of caryophyllenic and norcaryophyllenic acids must have recalled the very first research interest in the terpene field since they are close relatives of pinic and norpinic acids.



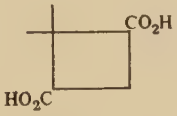
caryophyllenic acid



norcaryophyllenic acid

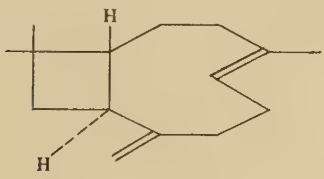


pinic acid

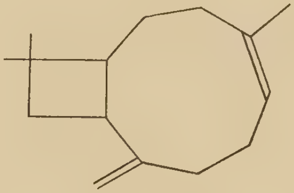


norpinic acid

The question as to what lies between the carbons of the carboxyl groups was not answered, though Ramage, especially, made important advances, until D. H. Barton and his colleagues entered the field (no attempt can be made here to assign relative credit to the various workers, Treibs, Sŏrm, Rydon, Ruzicka and Reid with their respective collaborators, were among those who contributed to the outcome). Barton has shown that caryophyllene is the *trans*- and *iso*caryophyllene the *cis*- isomer of the following structure. The geometric isomerism concerns the endocyclic double bond.

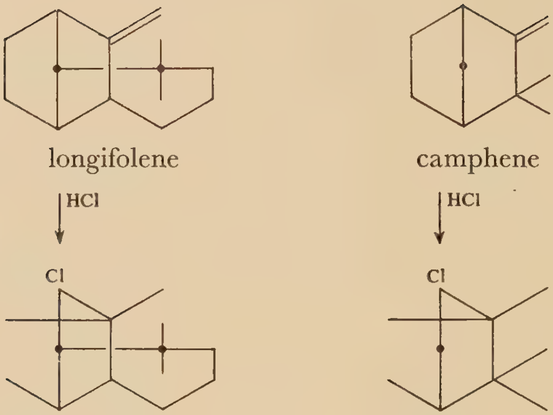


caryophyllene



*iso*caryophyllene

A tricyclic sesquiterpene, termed longifolene, was shown by Simonsen (1920) to occur in the oleo-resins of *Pinus longifolia* and two other Indian species. This he carefully characterized especially by the preparation of crystalline hydrohalides. The chemistry of this intriguing hydrocarbon was studied by Simonsen alone and later in collaboration with Bradfield and Francis. Unfortunately, the excellent work was misinterpreted in some respects though foundations for later investigators were well and truly laid. The structure shown below was finally established by elegant X-ray crystal analysis due to Moffet and Rogers (1953) and the analogy to camphene applies also to the molecular rearrangements noted in the longifolene series. The structure has been confirmed by the work of Dupont and his collaborators (1948-1954), one of whom, namely Ourisson (1954) has demonstrated the relation of longifolene to camphene on the basis of molecular rotations.



longifolene → longifolene hydrochloride

camphene → *iso*bornyl chloride

In late 1942 Simonsen was appointed Director of Research of the newly formed Colonial Products Research Council of which Lord Hankey was the first Chairman. He had office accommodation at the Imperial Institute and Lt.-Col. H. J. Holman became his personal assistant. Sir Norman Haworth and the writer were original members of the Council and we were greatly impressed by the energy and administrative ability which Simonsen then displayed under entirely new conditions.

He served also as a member of Sir Ian Heilbron's Insecticide Panel and was Chairman of its peace-time successor—The Interdepartmental Insecticide Committee and the Stored Foods Committee. He became a member of the Agricultural Research Council in 1945 and was U.K. delegate to the F.A.O. Specialists Committee meeting held in London in 1947.

In 1944 I had the privilege of visiting U.S.A. and the Caribbean with Simonsen; we were accompanied on part of the journey by Dr Alexander King at that time head of the B.C.S.O., Washington. This trip, under Government auspices, had several important results. One was the establishment of a Microbiological Institute in Trinidad, another less directly was the encouragement of a Sugar Research Association in the industry and still another, perhaps, a further stage in the re-assessment of the work of the Imperial College of Agriculture, Trinidad.

A visit to Jamaica was projected but all available planes were grounded and in substitution we decided to go to British Guiana. There we had consultations with the Deputy Governor and other important personages in industry and government. We noted the possibilities for chemical industry in the country where there is potential water power and plentiful bauxite of excellent quality. Above all we were struck by the overriding importance of the plague of malaria in the fertile coastal strip. We strongly recommended a D.D.T. experiment and a cable was sent to Sir Ian Heilbron. The work under Symes was ably carried out with the help of Professor Buxton and experts from U.S.A. New methods were evolved to suit the case and the experiment was completely successful. The infantile mortality rate was reduced to such an extent that a new labour problem may menace the future. Presumably this situation can be alleviated by an extension of industry.

In September-October 1946, Simonsen, in company with Heilbron, visited the East African Colonies and at the invitation of the Union Government also went to S. Africa. Under his direction the Colonial Products Research Council facilitated much academic research by the collection and free provision of plant material from colonial sources. He was also instrumental in fostering laboratory research on the utilization of sucrose (Haworth, Wiggins) leading, as one example, to a convenient process for the manufacture of laevulinic acid. This acid has recently found application as an intermediate for the manufacture of resins having special properties.

In addition to his original scientific work and his administrative activities, Simonsen will long be remembered for his comprehensive treatise on *The terpenes.*

This appeared first in 1931 in two volumes and a new and enlarged edition in five volumes was published from 1947-1957. In fact the reading of the proof of Vol. V was completed only a short time before he died. Dr L. N. Owen was joint author of the first two volumes of the 2nd edition (1947, 1949). Vol. III (with Dr D. H. R. Barton) appeared in 1952 and contains some addenda to Vols I and II. In Vols IV (1956) and V (1957), Dr W. C. J. Ross was joint author.

The whole work is of inestimable value as a standard of reference but it is more than a mere dictionary. The subject matter is developed throughout in the manner of a university professor who is at once a lucid teacher and the possessor of a logical mind and an appreciation of what is essential to the argument and exposition.

Students will find *The terpenes* very well worth reading as illuminating scientific literature and not just in order to dig out certain facts. The subject is so intricate that errors must be unavoidable but there exists no better starting point for a study of the original literature.

In 1945, Sir Norman Haworth, President of the Chemical Society, asked Simonsen to assume the arduous duties of Hon. Secretary. This request was made in view of the approaching Centenary Celebrations postponed from 1941. Simonsen held this office until 1949 and gave wise counsel during the difficult post-war period. He served on the Council of the Royal Society (1942-4) and in 1945 was U.K. delegate to a scientific conference initiated by the Society. He led the Colonial Delegation at the Commonwealth Official Scientific Conference which followed. In April 1948, during a memorable visit to Sweden and Finland, Simonsen met the ageing Professor G. Komppa, who attended a lecture that he delivered in Helsinki. Komppa, first to synthesize camphor, bears an honoured name in terpene chemical records—he appeared very much affected by the loss of his laboratory and valuable specimens.

In 1949 Simonsen was knighted (New Year's Honours) and was the first recipient of the Fritzsche Award of a Gold Medal and $1000 from the American Chemical Society. This was for his distinguished work on essential oils. He went to Atlantic City to deliver his Award address in September and then to Ottawa where he also lectured at the National Research Council's Laboratory. He was elected F.R.S. in 1932 and awarded the Davy Medal in 1950. Other honours were Hon. D.Sc. (Birmingham and Malaya); Hon. LL.D. (St Andrews) and the Kaiser-i-Hind silver medal (1921).

Sir John had a great gift for friendship and was a most lovable person. He was transparently sincere in all his relations, personal and scientific, and quite incapable of any mean action.

His scientific work was throughout of excellent calibre and absolutely reliable. He was in several cases unfortunately misled by unusual rearrangements and he did not have the advantage which certain modern techniques, especially infra-red spectrography and chromatography, have conferred on present-day workers in the difficult fields that he sought to cultivate. Nevertheless, much of his output is of fundamental importance and finds a place in the permanent literature.

He had many research collaborators and pupils who later achieved distinction. Of these the most eminent is Professor E. R. H. Jones, F.R.S., at present Waynflete Professor of Chemistry, Oxford University. Professor Jones has written as follows:

> 'In his lectures and in his visits, often in his shirt sleeves, to the laboratories, Simonsen managed to convey to the undergraduates his keen and very personal interest in organic chemistry. As a research supervisor he was an exacting taskmaster, setting high standards in endeavour and achievement. Apart from lectures and one or two tours around the research laboratories, his whole day, from half past eight, was spent at the bench, with a short interval for a sandwich lunch while perusing a copy of the most recently arrived journal. Supervision, however, was not just a matter of advice and encouragement but involved real practical assistance and much was learned by watching him carry out a distillation or coax an unpromising gum to yield crystals. Formality was the rule in his contacts with research students, non-chemical activities were rarely discussed, and it was only after leaving Bangor that more personal relationships were established. Then one gradually came to realize his deep sense of loyalty to his former collaborators, his concern for their welfare, and his sustained interest in their activities.'

He was active in literary and chemical pursuits up to the time of his sudden and unexpected death at his home in Putney on 20 February 1957.

He was a warm-hearted and generous man as well as a scientist and administrator of the first rank.

The happiness of his marriage was patent to all his friends and Sir John paid many warm tributes to the help always given him by his wife.

R. Robinson

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J. Swinburne

JAMES SWINBURNE

1858-1958

James Swinburne lived to be over one hundred years of age, the third Fellow of the Royal Society to do this. The first was Sir Moses Haim Montefiore who was born in Leghorn on 24 October 1784 and died in this country on 28 July 1885, having settled here as a young man. He was elected into the Society on 16 June 1836. He is still well remembered as an outstanding philanthropist and a fearless defender of his fellow Jews all over the world. The second was Henry Nicholas Ridley the botanist who died in 1956. One other Fellow who died a few days before his hundredth birthday might well be mentioned; he was Bernard le Bovier de Fontenelle, Secretary of the Académie des Sciences and later President of that body. He was born on 11 February 1657 and died on 9 January 1757.

Family, Early Life and Background

The Swinburne family is an ancient one as a glance at *Burke's Peerage* will show; they are essentially Northumbrian. The baronetcy dates from 1660. John Swinburne, father of the first Baronet, was promised a baronetcy by Charles I but the patent of creation never passed the seal. He died in 1652, eight years before the Restoration. His son, also John Swinburne, was created a Baronet in 1660 and is called in the patent 'Virum patrimonio censu et morum probitate spectabilem'.

Swinburne on his father's side was descended from Hotspur, of whom he dryly remarks in his personal record 'was a lively member of society but not noticeably scientific'. One of Swinburne's ancestors (Sir John Swinburne, Bart.) was a Fellow of the Society elected 26 February 1818. Like so many of his contemporaries of the period he was an ardent antiquarian and F.S.A. Humphry Davy was his proposer for the Society. He founded the Antiquarian Society of Newcastle upon Tyne, closely modelled on the L.S.A., and during his lifetime maintained the most friendly intercourse between the two Societies. He, like our Swinburne, lived to a great age, dying a few weeks short of his entry into his hundredth year.

Swinburne's father was Captain T. A. Swinburne, R.N. He served in the Syrian campaign of 1840, became mate in 1844, and after service on the North American, West Indies, and Mediterranean stations was promoted Lieutenant in 1846. He saw service in the Black Sea during the Russian War of 1854/5 and was present at the naval attack on Sebastopol. He was advanced to Commander in 1861. In 1864 he became Inspecting Officer of Coastguard. He retired with the rank of Captain in 1876 and died at

Beauregard in Guernsey on 7 December 1893. He was awarded the Syrian, Crimean and Turkish medals, the Sebastopol Clasp and the 5th class of the Medjidieh.

Swinburne's mother was the daughter of Captain Edward Fraser of Gortuleg. Swinburne was born in Inverness and brought up on the lonely little island 'Eilean Shona' in Loch Moidart at the southwest corner of Inverness-shire, he was the third of six children, all brothers. Eilean Shona means 'Look out Island', Shona being a Gaelic transliteration of a Norse word. I must here thank some of my colleagues in the Professors Dining Club at University College London who solved this difficulty for me.

Eilean Shona where Swinburne spent his youth is about 1200 acres in area, now well wooded with trees planted by his father who also owned Muck, the Isle of Pigs, where he grazed his sheep. Eilean Shona is remote now but in the sixties of last century it was almost out of this world. Stores came by 'puffer' to Salen, Loch Sunart and had to be brought to Dorlinn, the nearest point on the mainland, six miles by cart. The family ferried them over to the island. As Swinburne says in his laconic record 'Early life largely on the sea in yacht or small boat'. The servants all spoke Gaelic and often had to be taught English. Swinburne spoke Gaelic as a child.

From Dorlinn to church at Aharacle is over three miles. Swinburne remembered at the age of five walking there and back enduring a three hour service and then rowing home. When the family went to church the boys pulled in their shirt sleeves with their Sunday jackets folded in the stern sheets.

His mother (Mary Fraser of Gortuleg, a house with many Jacobite connexions) was very absent-minded, which made life somewhat hazardous. She in her large rambling house was constantly changing boys and furniture from room to room. It was her habit to visit each boy at bedtime and sing a hymn to him before bidding him good night. On one occasion, having forgotten that the previous day she had changed the rooms round, when she went to kiss her son goodnight, she found she had entered the room of a red-bearded neighbour who had come from Mull to buy sheep from her husband.

On another occasion she gave the keeper what she thought was a stiff dram of whiskey. He thanked her profusely and next day, having discovered that she had given him vinegar by mistake, and apologizing to him, the natural gentleman said, 'It was the best vinegar I ever tasted.' None of the clocks would go until Swinburne at the age of 12, showing his early mechanical ability, mended them all. It was a strange but very enjoyable happy-go-lucky youth for Swinburne, who had the most affectionate memories of the island. In one of his last letters to his daughter he said, 'If you go there you will not find anything that I have described, only a dear old house which I mean to haunt lovingly when I pass over.'

In 1870 Swinburne went to Clifton College where, as he says in his record, 'Education ordinary except that Rodnell, Perry and Tilden were masters.'

There can be no reasonable doubt where and when Swinburne was fired with the idea of becoming an engineer and became interested—he was a polymath if ever there was one—in the natural sciences; it was at Clifton. Tilden was a chemist later to become Sir William Tilden, F.R.S. and Davy Medallist. Perry was a graduate of what is now the Queen's University of Belfast, an accomplished mathematician, physicist and engineer, later to become Dean of the City and Guilds and Professor of Engineering there. He designed the Hammersmith Bridge. Rodnell was a chemist and a Fellow of the Royal Astronomical Society.

Very soon after leaving school Swinburne was apprenticed to a locomotive works in Manchester. Here he undoubtedly developed his remarkable skill with his hands which was already inherent. Later he went to a Tyneside works and became interested in the rising electrical industry. Now we begin to get on firmer ground. In 1880 (I now quote from Sir Kenneth Swan Q.C.'s life of his father Sir Joseph Swan, F.R.S.):

'The establishment of a lamp factory in France having been decided upon, he looked round for someone competent to superintend this undertaking.

'It was not easy to find anyone suitable for the job; for electrical engineers did not exist in those days. Again, however, fortune favoured him in his choice. A young mechanical engineer, James Swinburne by name, now a Fellow of the Royal Society and eminent as an electrician, consulting engineer and expert witness, was introduced to him and Swan perceiving him to be a man of parts asked him to undertake it. So Swinburne was given three weeks intensive training at the lamp factory at Newcastle upon Tyne and was then sent out to set up a similar factory in Paris.

'He was commissioned to call at Antwerp on the way to make a lamp there in order to comply with the Belgian patent law requirements. It took three weeks to make that lamp under the conditions then obtaining. A bit of a sawmill boarded off served as the works. The plant was a Gramme (series wound) dynamo whose speed varied from 500 to 1500 revolutions from minute to minute. The lamp was finally completed. It cost about £100 and was sold at quite another price to the owner of the saw mill with a caution that he should above all things avoid running it.' The latter is probably a typical piece of Swinburnian humour.

When Swinburne arrived in Paris the French principals were dismayed by his youthful appearance. So he immediately engaged the services of an elderly and most dignified looking Frenchman and took him to the factory saying 'This is my manager' with which they were well satisfied!

The factory in Paris had to surmount all the difficulties already experienced in Newcastle but by the end of 1881 France was being supplied with French-made Swan lamps. Later, still in the service of Swan, he started a lamp factory in Boston, Mass.

About 1885 he joined Crompton's dynamo works as a technical assistant. It is not entirely idle to speculate on his first meeting with the redoubtable Colonel R. E. Crompton, F.R.S. They were both the sons of naval officers who were near contemporaries. Crompton had managed to get himself into the Royal Navy at the age of eleven and had served at Sebastopol (later as a soldier doing pioneer mechanizing of the Army in India) and who introduced searchlights into the Navy. Swinburne eventually became manager of the dynamo works.

In 1899 he set up as a consultant in Victoria Street and became very successful. He had his own workshop beautifully equipped, with a place for everything, and everything in its place, and a chemical and physical laboratory in which he was constantly at work.

At about the beginning of his practice as consultant Swinburne made an epoch-making suggestion. I now quote from Sir Kenneth Swan's book:

'Mr Swinburne has supplied the following interesting piece of history with reference to the viscose basis of artificial silk manufacture. About 1899, shortly after Stearn had commenced the manufacture of artificial lamps at Kew, Swinburne met him and asked him how he made his filaments. Stearn told him "From chloride of zinc solution of cellulose". "Why don't you try viscose", suggested Swinburne, "it is much less tricky to work." After a few moments Stearn came back with a reel of filament made from viscose. Swinburne had seen in the Inventions Exhibition of 1885 Swan's exhibit of artificial silk made from carbon filament material, and possibly some recollection of this passed through his mind and prompted the further suggestion. "*Why don't you make artificial silk from cellulose?*" Stearn did not know anything about artificial silk manufacture so the Chardonnet process was explained to him.

'In a month or two he came back to show Swinburne a beautiful sample of artificial silk and offered Swinburne a share in the syndicate he was starting to work the process. After carrying on the manufacture for a short time, the syndicate was taken over by Messrs Courtaulds, and this was the beginning of the enormous industry which that enterprising firm has so successfully developed.'

We can now turn to the beginning of Swinburne's interest in plastics. I now quote from Mr T. J. Fielding's *History of Bakelite Ltd.*:

'In his Presidential Address to the Institution of Electrical Engineers delivered on 4 December 1902 Mr James Swinburne surveyed the field of heavy electrical engineering. On the subject of cables he was critical. "The conductor itself can hardly be improved, but there is great room for improvement in the insulation. It is largely the isulation of the cables that limits our pressures and therefore our distance of transmission."

'At the time that he expressed this view on the necessity for the improvement in the insulation it is certain that Mr Swinburne (now Sir James Swinburne, Bart., F.R.S.) did not foresee the development of the modern plastics industry which he subsequently did so much to pioneer. It is equally certain, however, that his engineering background and his awareness of the deficiencies of existing materials made him peculiarly receptive to any suggestion for development of new materials.

'At that time Sir James was a consulting engineer with an office at 82 Victoria Street, London, and during the course of his professional experience he saw a specimen of a resin obtained by an Austrian chemist named Luft from the reaction of the chemicals phenol and formaldehyde. The resin was handed to Swinburne as something that was novel but regrettably useless.' Swinburne said that it was like frozen beer. Continuing the quotation: 'In its then existing state the resin was useless, but Swinburne came to the conclusion that the process resulting in Luft's resin could be made to yield a really useful material. He therefore determined to undertake research into the process, and for this purpose formed, in 1904, the Fireproof Celluloid Syndicate Ltd. The phenol formaldehyde reaction was not even new at this time. In 1872 Baeyer had shown that the interaction of the two yielded a hard resinous substance, but there had been little systematic examination of the reaction. Swinburne was pioneering in this country and Dr L. H. Baekeland, a Belgian chemist working in the United States, had begun a detailed investigation of the effect. Baekeland had already acquired a considerable reputation by his discovery of Velox gaslight photographic printing paper.

'If phenol and formaldehyde are brought together in a test tube under conditions of normal room temperature, there is no immediate change, although they will react over a period of months. If heat is applied to the test tube the reaction is accelerated. By the introduction of a catalyst—either acid or alkali—"the hard resinous substance" can be produced in a matter of hours. These experiments were conducted by Swinburne, who discovered that caustic soda was a suitable catalyst. He did not, however, file his patent for this method of producing synthetic resin for two or three years in the hope that he would discover a better method, and it was obviously undesirable to publicize the trend of his research before he was sure he was on the right track. When he did file his patent in 1907 it was only to find he had been anticipated by Baekeland by one day.'

Baekeland's patent was singularly complete and there is little reason to doubt that Swinburne immediately saw the writing on the wall. The Fireproof Celluloid Syndicate however carried on, Baekeland had swept the solid field, but one of their resins made from phenol and formaldehyde gave a good hard lacquer suitable for the protection of brass and similar polished surfaces. This lacquer was named Damard. I have been working on Swinburne for months now and if this is not a portmanteau word of 'Dam hard' showing Swinburne's humour it will be surprising.

The syndicate marketed this material under the name of Damard Lacquer Company. Two years later the syndicate was wound up and all rights and personnel were transferred to the Damard Lacquer Company. This was in 1910.

The above is mainly abbreviated from Fielding's book. Now to quote Fielding again verbatim.

'Although the Damard Lacquer was an extremely good product it is not to be imagined that the new company was immediately inundated with orders. Curiously enough the most important success on the sales side was orders from the United States, which in view of Baekeland's work was a somewhat pert example of sending coals to Newcastle and selling them. American orders were becoming too large to be met by shipments from England and the company daringly decided to open a factory in the United States.'

The Damard Lacquer Company started production in the autumn of 1912. The outbreak of war gave them a great fillip in this country but caused their American business to decline, and in 1916 they decided to close it down. This had the fortunate result of Baekeland meeting Swinburne with whom he formed a friendship lasting to the end of his life; and as Baekeland said many years later, he was lucky enough eventually to establish friendships with his former rivals, excellent men whom he counted as his dearest friends. Eventually Baekeland acquired the Damard Lacquer Company and several others when Bakelite Ltd was formed in this country. Swinburne was the first chairman, in which office he continued till 1948. He then retired with the office of Honorary Life President.

On his retirement he remarked to Mr H. V. Potter, his managing director and successor, that he did not propose to turn into a cabbage but would devote himself to his former interests in horology. This will be developed later in an account of his miscellaneous activities which are quite astonishing. On his hundredth birthday he received a telegram from H.M. the Queen, an address from the Royal Society and innumerable congratulations from learned bodies and his friends. He died a month later.

Swinburne as an Electrical Engineer

In spite of considerable efforts it has proved impossible to pin-point with any exactitude Swinburne's share in the growth of the Electrical Industry. First of all, to quote Swinburne himself from his 'record' (the issue of the 'personal record' together with a request for Fellows to fill in details of their career according to a plan took place in the Presidency of Sir Henry Dale in 1941), Swinburne says:

'The Royal Society naturally wants to have particulars. My difficulty is that the work I did in the early development of electrical engineering, and its results, are now commonplace knowledge of elementary students, and are now not worth recording. In those days there were no text books, no technical knowledge and no good instruments. Some time ago realizing that I was getting on in life and might save my executors much time and trouble, I sent all the printed papers written by me and anything about me, together with all out-of-date scientific and technical books, to be made into bomb covers. There was a great mass of stuff from technical papers and scientific papers and proceedings; but I do not think it will be missed. This clear-out was before I had received the President's excellent suggestion.'

From internal evidence this was in all probability written in the year 1948 or 1949, that is to say when Swinburne was over ninety. The historical evidence is interesting and also throws light on the matter in the late seventies and early eighties when electricity, especially electric lighting, was very much in the public mind. *Punch*, as always a good mirror of contemporary events, carried a cartoon about the year 1880 showing two giants who look like Old King Coal and Neptune standing over the cradle of an infant imbibing nourishment from a bottle labelled 'Electricity'. The writer has a vivid recollection of talking to a near contemporary of Crompton's, long since dead, who told him of the piecemeal way in which electricity invaded the wealthy quarters of London for the electric lighting of private houses, frequently from a generating station in a stable which would light the whole of a square. But for the Electric Lighting Act of 1882 it is probable that something approaching the 'Railway Mania' would have developed. This Act, though deplored by some as a brake on progress, undoubtedly had a good and steadying effect in the long run.

The best evidence of the effect of Swinburne's ability and the effect he had on his scientific contemporaries is afforded by the certificate proposing him for the Royal Society which is quoted in full below. Today it sounds like voices from another world. The certificate is dated 29 December 1905. It was read on 18 January 1906 and Swinburne was elected on 3 May 1906. 'Has been intimately connected with the development of the theory of dynamo design since dynamo machines began to be manufactured on a large scale in this country. Developed the theory of armature reaction in direct current machines, and in alternating current dynamos and motors (originated by Lord Kelvin); worked also largely at the development of the theory of alternating-current measuring instruments; also at Clark's standard cells; investigated the action of various kinds of secondary batteries with lead and other electrodes; investigated questions of high vacua and methods of measuring very small pressures; has been for a number of years intimately connected with the applications of the sciences of physics and chemistry to industrial purposes and more especially with the theoretical work on electricity and electrochemistry specially arising out of these applications. Has published the following papers among others:

"Experiments on secondary cells" (Inst. E.E. 1886)
"Method of measuring electrical induction" (Physical Soc. 1887)
"Design of transformers" (Brit. Ass. 1889)
"Dynamos and motors" (Inst. E.E. 1890)
"High vacua" (Brit. Ass. 1890)
"Alternating current condensers" (Physical Soc. 1890)
"Alternating current distribution" (Inst. E.E. 1891)
"Some points in electrolysis" (Physical Soc. 1891)
"The electrometer as Watt meter" (*ibid.*)
"Causes of variation of Clark's standard cells" (Brit. Ass. 1891)
"Measurement of liquid resistances" (*ibid.*)
"Electrical measuring instruments" (Inst. C.E. 1892)

"Potentiometer for alternating currents" (Physical Soc. 1894)
" 'Hunting' of governed engines" (Brit. Ass. 1894)
"Nernst's electric light" (Soc. Arts 1899)
"Electric railways" (Inst. E.E. 1901)
"Chlorine smelting" (Faraday Soc. 1903)
"Reversibility of thermodynamics" (Brit. Ass. 1903)
"Entropy" (Book 1903)
"Chlorine in metallurgy" (Chicago 1904)
"Theory of electricity" (Inst. C.E. Students' address 1905)

'Original editor of the Physical Society's scientific abstracts; a past-President of the Institution of Electrical Engineers; Vice-President of the Faraday Society and a Member of the Institution of Civil Engineers.'

The certificate was signed by Silvanus P. Thompson, W. H. Preece, Kelvin, Alexander Kenworthy, J. Norman Collie, Fred T. Trouton, J. A. Ewing, W. C. Unwin, J. Wolfe Barry, M. H. White, Oliver Lodge, and William Crookes. Sir William Ramsay signed from general knowledge. This is an illuminating expression of contemporary and very distinguished opinion.

SWINBURNE AS AN EXPERT WITNESS

This can be dealt with almost summarily. I now quote from *The Times* of 28 February 1958. 'Referring to the hundredth birthday today of Sir James Swinburne the consulting engineer, Mr Justice Lloyd-Jacob said in the Chancery Division that he would like to acknowledge the tremendous contribution Sir James has made to our patent jurisprudence in acting as a distinguished expert witness over so long a period.' The above tribute of the learned judge is remarkable to say the least of it if one remembers the hoary legal, somewhat sardonic, classification of witnesses as liars, great liars, experts and my learned brother so and so. On top of this there is the list of the Chartered Institute of Patent Agents of their honorary Fellows. Other than their own members we have Lord Alverstone, Lord Moulton, Lord Parker, judges of very great eminence, together with Sir Duncan Levy, Q.C., and Sir Kenneth Swan, Q.C. Swinburne is included in this galaxy of legal talent, where there are only six names. The reason is not far to seek. Swinburne in a manner of speaking did not give evidence at all; he gave a kind of judgement, his witnesses being all the experimental facts very often obtained by himself in his own laboratory, suppressing nothing whatsoever. Sometimes he made his client's blood run cold. He was tremendously successful. It was said of him in his latter years that to engage Swinburne was to be successful. The proviso, of course, is, would Swinburne take you? He would have nothing to do with a case unless he was perfectly satisfied that it was a good and honest one. The *New Scientist* of 27 February 1958 contained a profile of Swinburne in which it is stated 'This was the age of litigation and soon James Swinburne had made for himself the most respected name in the country for clear unshakable and completely honest evidence. His candour and sense of humour were such that any firm knowing its case to be on slippery ground would avoid employing him.'

Sir Kenneth Swan, son of the electrical engineer, still practises as a barrister and has known Sir James for seventy years. He recalls 'The demand for his services as an expert witness constantly recurred and extended to other fields quite outside electrical engineering; for example, in the pneumatic tyre legislation of the Welch Patent, in the numerous actions brought to establish the Auer von Welsbach patent for incandescent gas mantles and in the Edison & Bell phonographic letigation. His evidence was eagerly sought in actions concerning inventions as different as soda syphons and golf balls.'

Sir Kenneth's clerk remembers Swinburne well. 'Some of these cases were marathons going on for weeks. Swinburne was sometimes under cross examination for four or five days on end, and some of the cross examining counsel of those days were brilliant. Sir Stafford Cripps was a regular. But they could never get Swinburne rattled. He was the best of them all.'

Miscellaneous Activities

During his very long prime Swinburne talked but little about himself and his past. He was always concerned with something to be done in the future. It was only when he became very old that he began to talk about his past and his youth. His son, the present baronet, fortunately remembers a great deal of it. He was concerned in some way with the construction of the 'twopenny tube'. He was involved in a mathematical dispute with Poincaré.

During the first war he visited Australia on some matter connected with government wireless installations. He stayed for one week only. An article appeared in the *Sydney Bulletin* complaining of his short stay and insisting that it cost Australia five pounds every time he smoked a cigar—to which he replied that as he was a non-smoker the taxpayer might rest content.

He was an excellent and most accomplished musician and set to music two of Tennyson's poems which were performed quite late in his life.

In his youth he toured Ireland with some extraordinary committee which included a judge apparently to report on lighthouses. He went to South Africa representing some engineering company to report on the advisability of installing some heavy machinery. He travelled into the interior on a narrow-gauge railway and reported that the machinery could not be got to the place in question as it had not been built! It was Johannesburg.

When very young he visited the U.S.A., made friends with the chief engineer of the ship on the return journey and was shown a rapidly spreading crack which was likely to stop her. He made a reasonable suggestion for stopping this which was adopted and which worked.

He built a large organ in his house. He used to read musical scores in the train camouflaged by the cover of a machine-tool maker's catalogue. He is believed to have written a thriller wherein the murder was committed by an artist who mixed Scheele's green in a toothpaste tube.

He conducted a series of experiments on the lethal effect of electric shocks. The present baronet, who is a doctor, was involved as a medical witness in a case concerning a workman who was electrocuted by household current; the widow's pension depended on the result. The mere fact that he was prepared to produce his father's evidence in court was sufficient to secure a verdict for the woman. Swinburne was scientific consultant to General Motors for a period. He was an authority on paper-bag machinery and on the metallurgy of safety razor blades. He knew a lot about naval gunnery and was on Lloyd George's committee of inventions in the first war. He invented an electric incubator and hatched a chicken in it. He wrote a book on tops. He was author of a book on population pressure which got a bad press, though a great deal in it would be generally recognized as true today. Referring to the criticisms he said, 'The pen is mightier than the sword and the jaw bone of an ass is mightier than either.' He wrote a book on the musical education of women which outraged feminists; in fact he was tilting at the dilettante education of the Victorian miss. He was mixed up in the raising of the *Egypt's* gold. Here the writer is on firm ground because he was behind the scenes himself and saw Swinburne in action. The story is set forth in the book *The Egypt's gold.* The work was carried out by the competent and heroic Italian crew and divers of the S.S. *Artiglio.* The British protagonists were the brothers Sandberg, consulting engineers of 40 Grosvenor Gardens. There were three of them, Peter the senior and Nils and Alec his brothers. They were a remarkable, enterprising and inventive trio. Peter was very friendly with de Rougement, the Chairman of Lloyd's. Hearing that the *Egypt* was regarded as a total loss he said he thought it could be raised and naturally obtained a very favourable contract. One of the three partners, the writer does not know which, was a constant travelling companion of Swinburne in the trains from Oxted. This led to his being called in, in what capacity the writer does not know, but in all probability as a friend. The writer met Swinburne many times in the extraordinary exciting developments culminating in the raising of almost all the bullion and was present when many difficulties were discussed and solved.

Finally, Swinburne invented those two very useful words 'rotor' and 'stator', and a glance at his patent list makes one wonder how one man could do so much.

His Character, Personality and Appearance

He was very good looking and in dress and appearance was what would now be called an Edwardian. He was utterly unimpressed by himself and never alluded to his past. He was neither short nor very tall. He was a man of the greatest integrity. He was usually laconic in speech but was an excellent talker and a very good listener. In his apprenticeship days he had but little money and had to live pretty hard, which gave him a life-long sympathy with poor people. He was most abstemious, being a teetotaller and a non-smoker. The respect and admiration of his contemporaries for him was tremendous.

His sense of humour was acute. He could be scathing but rarely was. He gave the impression of complete imperturbability. Like all humourists he looked rather grave, but there was always a twinkle in his eye when he talked. Mr H. V. Potter, who followed him as chairman of Bakelite Ltd, informs me that he was not wildly interested in the commercial side of the business and left it mostly to him, but he was anything from a couple of years to a decade in front of his time and was passionately interested in developments and the future.

His son informs me that he can hardly remember him ever being in a temper. His manners as might well be expected were beautiful and courteous. His vigour and constitution were astonishing. He always went up stairs two at a time and did not like lifts. To sum him up we may say he was the Victorian gentleman at his very best and a most lovable genius (*Semel et semper*, as the family motto has it).

Swinburne married twice. First in 1886 Ellen Wilson, the daughter of Dr Wilson of Newcastle upon Tyne, by whom he had two sons, Anthony born in 1887 who died in 1953, and Spearman Charles born in 1893, the present baronet who by profession is a doctor and is married to Millicent, widow of G. F. Fenton, the Connaught Rangers, and elder daughter of Lt.-Col. E. H. Montresor the Royal Sussex Regiment. Swinburne's first wife died in 1893. Swinburne married again in 1898, his wife being Lilian Gilchrist Carey, the daughter of Sir Godfrey Carey, Bailiff of Guernsey. By her he had two daughters, Ida born in 1899 now the wife of Commander D. H. Rainier, D.S.C., R.N.(Ret.), who now lives in the Highlands, and Marjorie born in 1904 who died in 1927.

Offices and Distinctions

Fellow of the Royal Society 1906.
President of the Institution of Electrical Engineers 1902-3 and for 73 years a corporate member, an unequalled record.
President of the Faraday Society.
President of the Junior Institution of Engineers.
Member of the Institution of Civil Engineers.
At one time member of the Institution of Mechanical Engineers.
President of the Plastics Institute 1937-8.
Member of the Council of the Royal Musical Association.
Member of the Physical Society.
Fellow of the Chemical Society.

The writer wishes to express his grateful thanks to the following:

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associate of Swinburne for many years. To Miss M. K. Weston, B.Sc.(Eng.), A.M.I.E.E., and Mr J. E. Smart of the Science Museum for much help and the loan of books and papers. To the Librarian of I.C.I. (Alkali) for help with the bibliography.

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ADDENDUM

It is the earnest hope of the writer that the information herewith might tempt some student of the history of science (and for the matter of that, human nature) to write a full length life of Swinburne. A tremendous, difficult, but worth-while task.

F. A. FREETH

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PATENTS IN THE NAME OF SIR JAMES SWINBURNE, F.R.S.

1883	2350	Electric cells or batteries
	5159	Incandescent electric lamps
1884.	1157	Induction coils
	1176	Dynamo-electric machines
	1178	Incandescent or glow lamps
	3999	Filaments for incandescent lamps
	4000	Filaments for incandescent lamps
	4070	Electric governors for dynamo machines
	4121	Incandescent electric lamps
	4271	Incandescent electric lamps
	4432	Plates for secondary cells or batteries
	6353	Marking glass or vitreous surfaces
	9935	Holders for electrical glowlamps
	12654	Electric governors
1885.	1423	Electric arc lamps
	3805	Electric governors
	3941	Dynamo-electric machines
	3942	Electric measuring instruments
	11421	Filaments for electric glow lamps
	11861	Electric glow lamps
	12001	Carbons for electric lamps
	15484	Electricity
1886.	5168	Electric lighting of trains
	11863	Electric arc lamps
	17120	Dynamo-electric machinery
1887.	6219	Induction apparatus
	6682	Electrical distribution by means of direct or alternating current for lifting purposes
	6754	Dynamo-electric machines
	12437	Electric lighting
	17599	Dynamo-electric machines
1888.	5811	Generating and distributing electric power.
	12437A	Electrical measuring instruments
	14095	Dynamo-electric machines and electric motors

1889	3424	Dynamo-electric machines
	8588	Dynamos and motors
	12236	Alternating electric currents
	12237	Electric transformers
1890.	878	Ventilating culverts for carrying electrical conductor
	962	Cars worked by electric accumulators
	3193	Transformer, electric measuring instruments
	11070	Electric measuring instruments
	13886	Distributing power by alternating electric currents
	19308	Electrical distribution of power
1891.	1639	Condensers for alternating electric current
	9103	Dynamo-electric machines
	9915	Electric measuring instruments
	18784	Electric measuring instruments
1892.	475	Electric testing apparatus
	5579	Dynamo-electric machines and electric motors
	11735	Dynamo-electric machines
	16919	Dynamo-electric machinery, alternating current motors, transformers, ozone and oxide of nitrogen, recovering tin from scrap, electro-plating
1893.	16307	Electric meters, dynamos; electrolysis of solutions; sodium and sodium substitutes; potassium cyanide, gold extraction and recovery of tin from scrap
1894.	14892	Jointing wrought iron pipes
	16278	Electric measuring instruments
1895.	7379	Working aluminium
	17766	Soldering aluminium
	22976	Electric telegraphs, organs, condensers; packing fragile articles
1896.	5041	Conductors for telegraphs, telephones
	10728	Generating acetylene from carbides
	18006	Coating metals
1897.	10829	Treating sulphide ores
	23099	Arc-lighting electric generators
1898.	3136	Treating sulphides
	3137	Treating sulphides
	3138	Separating metals
	10829A	Treating sulphide ores
	19949	Signalling
1899.	5795	Electric light
	14010	Producing radiation and light
	14278	Treating sulphide ores
	23322	Treating sulphide ores
	23323	Treating sulphide ores
1900.	5796	Decomposing oxides
	5797	Breaking-up fused chloride
	5798	Separating and purifying metals
	6165	Incandsecent lamps
	10940	Incandescent lamps
	14971	Incandescent electric lamps
	17611	Treating ores
	18269	Electrolysis of fused salts
1901.	4448	Reduction of metals
	5041	Incandescent electric lamps
	6857	Treating zinc chloride

1901.	7028	Extracting metals
	12991	Electric lamps
1902.	2990	Incandescent electric lamps
	4473	Electric railways
	6347	Electric traction
	6766	Electric traction
	19031	Engine governors
	26717	Electric locomotives
	26718	Dynamo-electric machines and motors
1903.	8328	Treating ores
	14613	Utilizing iron chloride
	26911	Treating ores
	26913	Telephone exchange system
1904.	16491	Treating ores
	16492	Treating ores
	17521	Treating ores
	29186	Treating antimony, arsenic, ores
1905.	2268	Electric working of mine shafts
	11793	Treating sulphide ores
	16671	Electric hoisting apparatus for mines
	17325	Treating ores
	21391	Electro deposition of zinc
	23449	Electric working of mine shafts
	23450	Treating poor ores
1906.	19509	Sulphide ores
1907.	9689	Incandescent lamps
1909.	27864	Organs
1910.	25603	Musical instruments
1912.	11408	Lacquer
1914.	24032	Concentrating solutions
	137572	Low temperature distillation of coal
	206211	Feed-water heaters
	235301	Extraction of dust, etc., from gases
	247265	Gyroscope compasses
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J.

FRANK TWYMAN

1876-1959

Frank Twyman was born in Canterbury on 17 November 1876. His father, George Edmund Twyman, had nine children, and Frank was the seventh child. George Twyman was a ropemaker, specializing in ropes, netting and jute articles for the hop trade. His sons used to help, and Frank cut out and sewed hop-pockets, and designed a machine for marking hop-pockets with their consecutive numbers. Frank Twyman attended the Simon Langton School, Canterbury, until September 1892 when he began a two-year course in electrical engineering at the Finsbury Technical College, where he studied under Sylvanus Thompson, John Perry and Meldola. While at Finsbury, he won the Siemens Scholarship to the Central Technical College, South Kensington (later to form part of Imperial College). While he was at school, he had proved so expert at passing examinations with a minimum of knowledge that 'in entering for the Intermediate B.Sc. examination I thought I could pass without knowing anything whatever. I was mistaken; so it is that my highest academic distinction is to have failed in both biology and chemistry in the Intermediate B.Sc. examination.' It is fair to add that at the time he was making a model of 'Duplex Telegraphy' for a lecture of Professor Ayrton's, and correcting the proofs of a new edition of one of Ayrton's books, and he attributed his failure in part to the conscientiousness with which he was doing these tasks.

In 1897 he published his first scientific paper (in collaboration with S. S. Spiers & W. L. Waters) on the 'Variations in the electromotive force of the H-form of Clark cells with temperature'. In July of the same year he left the Central Technical College, and obtained a post in the Fowler Waring Cables Company at North Woolwich. This company was later bought by the Weston Electric Cables Company. He was much disappointed at the work, which he found trivial in the extreme. In these days telephone cables laid underground, consisted of 306 16-gauge copper wires, wrapped in paper, and coated with lead. Twyman's work was to test the insulation, and other properties of the cables, and prove the correct connexion under the eye of an inspector of the purchaser of the cable. As one of the jointers working in his territory was sometimes under the influence of drink, this frequently proved an ordeal. At the end of January 1898, after a period of three months' continuous night work, he felt in need of a rest, and he met a friend (E. W. Marchant, later Professor of Electrical Engineering at Liverpool University) who told him that Otto Hilger was in need of an assistant. Twyman was prepared to go for nothing, to have light and interesting work, but Otto

Hilger paid him 25s. a week to join him, and on 2 February 1898, he began at Hilger's. In after years, Twyman often said that he came to Hilger's for a rest, but that it proved a most complicated way of earning a living. In 1902, Otto died and Twyman succeeded him, and in 1904 the company was incorporated under the title of Adam Hilger Ltd, with Twyman as Managing Director, a post he held until 1946. In 1948 the firm amalgamated with E. R. Watts & Son, and Twyman served as a director of Hilger & Watts until 1952, when he became Technical Adviser until his death.

(The father of the Hilger brothers was Master of the Mint at Darmstadt. Adam Hilger worked for a time in Paris, under the direct supervision of Foucault, and he constructed many instruments for the Paris Observatory. He came to London in 1870 and worked for John Browning until in 1874 he set up business on his own account. Most of his first orders came from Lt.-Col. Archibald Campbell, afterwards Lord Blythswood. Adam died in 1897, and the control passed to his brother Otto, who had meanwhile been assisting Lord Blythswood as mechanic in constructing the celebrated ruling engine for diffraction gratings.)

When Twyman joined Otto Hilger, the chief output consisted of spectroscopes, which in 1896 were sold for £10—in sharp contrast with the elaborate automatic instruments now made for the rapid analysis of metals in industry, which may cost as much as £15000. Twyman began by working at the bench, helping with correspondence, making calculations, and testing optical work and adjusting all the instruments. He brought into the firm the use of proof plates for testing the surfaces of the optical components. Until 1910 he made all the designs and working drawings for new instruments, and superintended the making of them.

Spectrochemical analysis

In 1902 Twyman took the initial steps in furtherance of what was to prove the chief aim of his life—to make spectrochemical analysis a matter of routine in industry. W. H. F. Talbot found in 1835 that lithium and strontium gave rise to spectra which differed from one another, and suggested that various metals could be analyzed by means of their spectra. Twenty-six years later, Kirchhoff and Bunsen made a spectroscope which became the standard form for many years, and analysis by the spectroscope was in frequent use in their laboratory, so that often they are credited with originating the idea. It was not taken up, however; there were many disappointments, largely because insufficient was then known about the nature of spectra—the possibility of different spectra arising from the same metal as a result of different stages of ionization, or of its being in atomic or molecular states was not imagined. A notable exception at the time when Twyman began was Sir Herbert Jackson, who encouraged him greatly. Twyman began by designing the wavelength spectrometer. This was a robust spectroscope with the collimator and telescope rigidly fixed at right angles to one another, and a constant-deviation prism which was rotated by a micrometer screw to change the spectral line seen centrally in the eyepiece. The screw terminated in a drum, on which the wavelengths were engraved. This type of instrument persists today, almost unchanged even in details, and it has been widely copied. This instrument was an immediate success, but not in the way intended, for it was bought by physicists and chemists all over the world, for teaching and research. It was neglected by analysts. Twyman considered that the neglect persisted because the optical components of this spectrometer were made of glass, so that it was restricted to work in the visible range. So in 1910 he tried again, this time using quartz so that the ultra-violet lines could be used as well as those in the visible, for the chief lines of analytical interest lie in the range from 2500 to 3500 Å. He made a sturdy instrument mounted on a girder framework with everything fixed, rightly considering that if he were to be successful in his aim, the instrument should be simple and robust. (A later model of this instrument was dropped from an upper window on to a flower-bed in Singapore, when the rapidity of the Japanese advance left no time for normal transport. It was buried, dug up at the close of hostilities, and found to be unharmed.) This time one analytical sale occurred—to the American Brass Company. In 1912 a larger quartz instrument was made, and three were sold to America—one to the Mellon Institute, one to the American Brass Company, and the third to the General Electric Company, Cleveland, Ohio. After eight years of vigorous use, the American Brass Company published a paper on the work done with the spectrograph and at last metallurgists began to take notice. One result was that Rosenhain, on returning from a visit to America, lectured in London on the strides being made in America in applying the spectrograph to analysis, unaware that the spectrographs concerned had been made in London.

The biggest advance towards the fulfilment of Twyman's hopes occurred when Zeiss in Germany and Bausch & Lomb in the U.S.A. made instruments closely similar to the medium quartz spectrograph, for they subsidized researchers in universities to use them and publish their results. Twyman offered to lend an instrument to a famous university metallurgist for a year to try what it could do, but without success. This, however, proved unnecessary, for the foreign propaganda was so successful that later many metallurgists purchased and used his spectrographs. By 1939 the country was so well equipped to test alloys that the Hilger factory was marked on German invasion maps as a key place to be taken over (incidentally it was misplaced on the map).

Other regions of the spectrum

In 1913 Twyman designed the first commercial infra-red spectrometer—a simple Wadsworth prism arrangement with concave mirrors. He did this against the advice of his friend Paschen, whose view was that the infra-red region would never be of any but strictly academic interest. In 1921 he made the first commercial vacuum grating instrument, which went to Bohr's laboratory in Copenhagen. This was followed by other normal incidence instruments, and still later a grazing incidence vacuum grating spectrometer.

In X-rays he interested himself in equipment (1923) for crystallography, and made an instrument designed by Müller at the Royal Institution. This in turn led to the making of other X-ray equipment, such as, for example, Laby's apparatus for investigating X-ray spectra.

Interferometers

The interference method of testing optical components had already been in use in the firm through Twyman's introduction of the use of the test-plate, making use of the thin air-film fringes. The most skilled worker making fine optics was the foreman of the optical shop, Alfred Green, who in the course of his work suggested to Twyman that it might be possible to modify the Michelson interferometer so that it could be used for the same purpose. Twyman worked with him upon this, supplying the necessary theoretical knowledge and the result was the Twyman-Green interferometer, an instrument which has done more to improve the quality of optical components than any other device. Later variants were designed specifically for testing camera lenses, and microscope objectives. As a result, Twyman received the Duddell Medal of the Physical Society in 1927, and the John Price Wetherill Medal of the Franklin Institute. The citation of the latter was 'the great scientific value of his interferometer for the testing of optical parts'. Zeiss bought one of each of the three interferometers in 1929, as well as the German patent rights. Dr G. Hansen of that firm wrote:

> 'The interferometers were used in Jena to an ever-increasing degree for the testing of optical parts. In particular, since the beginning of 1930, such parts for all quartz and glass spectrographs in my department were so tested. Further, particular lens systems were frequently examined interferometrically when it was desired to obtain quickly an exact determination of the degree of correction. Later the instruments supplied by your firm no longer sufficed and we then ourselves made similar interferometers for our testing. The large interferometer for photographic objectives was used in the correction of the prisms of the tower spectrograph of the Einstein Tower in Potsdam.'

One can well believe that Zeiss needed more of these instruments than the first three: in the optics shop in Camden Town twelve instruments are continually in use. Another interferometric device made by Twyman was an early copy of Michelson's echelon. Inspired by reading Michelson's account of his work in 1889, Twyman spoke of it to Lord Blythswood, who promptly ordered one so that the money for the venture could be forthcoming. The echelon plates were cut from plates of 13 in. diameter and 1 cm thick. At first he used a Fizeau interferoscope made to Michelson's design, for testing plane parallel plates, but later replaced it by a design of his own which was more convenient for marking high spots. He showed his echelon at a conversazione, which Michelson himself attended and where he was surprised to see a good example of his own apparatus in Europe.

Glass-working

It was natural for Twyman to be keenly interested in glass, and this interest was quickened by the First World War. Optical glass had been obtained principally from Germany (Schott), with some from France. A year or so before the war, the supply from Germany ceased. Twyman sought help from Chance Bros in Smethwick, with the result that a supply was assured, though insufficient for all the needs of industry. The quality was good, but the annealing was done by traditional methods, not suitable to glass for optical purposes. He persuaded Chance Bros to pay for the apparatus which he needed for investigation, and he worked during 1915 on finding the best temperatures at which to anneal for different types of glass. In January 1916 he was able to tell them the best annealing temperatures for ten of their important glasses, having established the law of variation in the viscosity of glass with temperature, in the appropriate range of temperatures. Next month, he was able to enunciate a general principle of annealing, leading to cooling schedules. In one case, the time required for annealing was reduced from three weeks to three days—of special importance to a country at war. A. J. Stobart, who was Managing Director of Chance Bros at the time, wrote a statement from which the following is taken:

'On the outbreak of war, the demand for optical glass from Messrs Chance Brothers became very great, and the anxiety of those acquainted with the position was intensified by the following points:

Annealing plant had to be remodelled and control system devised.
Annealing temperatures had to be determined.
Cooling rates for best annealing fully investigated.

With all the difficulties and pressure for increased output, I, as Managing Director of Messrs Chance's Optical Department, did not hesitate to accept the assistance of Mr Twyman in regard to annealing matters, coming as it did at a most serious and critical time. By the use of his instrument, he determined the annealing temperature for the various glasses produced, and this information greatly facilitated the output as every moment saved was of vital importance to the country. Owing to the shortage of chemicals, even the standard glasses which had been produced in this country for years had to be changed, and by such a procedure the annealing range was altered, so that it may be taken that every type of glass produced for war purposes had its annealing temperatures determined by Mr Twyman and on the instrument devised by him.'

Twyman embodied his knowledge of glass working in his book *Prism and lens making* which was published in 1943. This is a classic book, and will stand for many years to come as a book of reference.

Miscellaneous work

He took a keen interest in economics, but was uninterested in politics unless they touched on economics. He wrote letters to *The Times* frequently, but none of them was published, and he used to say that he was considering publishing them as a book. This in fact he did with regard to some of the letters, in a pamphlet entitled *Some new proposals for the amelioration of the balance of payment position*, published in 1953. He considered that more provision should have been made for increased production after the war by using large derequisitioned premises for factories, that for young people continuation classes while in employment would have been a better step than raising the school-leaving age, and that more foreign workers should have been brought into the country. Further he wished more of the 'over 60's' to be given suitable employment, releasing younger people for more robust employment. He also wanted some encouragement of investment for those who spend money in gambling. Some of these proposals have in part been adopted, namely the 'sandwich' schemes and part-time classes for young people, and in another direction, the establishment of Premium Bonds. He was alive to the necessity of the training of apprentices, and in 1944 published a little book, called *Apprenticeship for a skilled trade*, which has had considerable influence on the development of technical training for industry. In this book he looked back nostalgically to the times when the guilds were important, and apprenticing youths a natural way of recruiting craftsmen. He thought that the universal application of the raising of the school-leaving age in the sense of children remaining at school was a mistake, and pressed for apprenticeship to be recognized as 'further education' within the meaning of the act. He was against the view that school was a good thing, and work a bad thing, and that therefore school should be prolonged and work delayed as long as possible. He believed that many young people are frustrated by being forced to go on with academic learning, and that it is a contributory cause to juvenile delinquency. As a manager, his method was to encourage everyone, however junior, to make decisions and act on them, provided he reported to his senior what he had done at the first opportunity.

He was a humble man, often surprised at his own success: he used to say that he was never frightened of making a fool of himself. He had an impish sense of humour, and would sometimes make ingenuous remarks for the joy of watching the reaction. The writer remembers being driven in his car by Twyman—always an apprehensive experience—when he steered somewhat erratically in front of a lorry. At the next red lights the lorry caught us up, and the driver commented pungently on Twyman's driving. His reply was 'I deserve all you say'.

In meetings Twyman delighted to let the most junior give his opinion first, but often his impatience made him cut the youth short. His apparent ingenuousness hid a clear appraisal of a problem, and having realized the way to proceed, he would begin the discussion by saying 'And what do *you* recommend?' When he had decided to increase someone's salary, he would call him in, and begin by drawing attention to some shortcoming, ending by the news of the increment.

He delighted in his absent-mindedness, and a favourite story of how he left some papers in the Underground during the war, some of them classified tenders. They were found and returned to him in a parcel by the Air Ministry, the book he had been reading on the top—Freud on 'Forgetfulness'. He was a very kindly person, loving to do good by stealth, and generous in his praise of assistants in their absence. He was fond of music, and for many years at lunch-time he shut himself in his office, with a glass of milk for his lunch, and most of the time played his violin. He used to say that he was practising to become a third-class amateur. Other interests were literature, gardening and the theatre, and he had an unusual memory of old music-hall songs.

Twyman was a Founder Member of The Institute of Physics and was elected a Fellow of the Royal Society in 1924.

The influence of his work has been great; the stimulation which he gave to the betterment of analysis throughout the world would alone be a fitting memorial for any man. Added to that, he has affected all work in optics by his improvements in the methods of manufacture of optical parts, the testing of them, and the improvement of the manufacture of the glass from which the components are made.

A. C. Menzies

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(1) 1897. (With S. S. Spiers & W. L. Waters.) Variations in the E.M.F. of the H-form of Clark cells with temperature. *Proc. Phys. Soc.* **16,** 38. Reprinted in *Phil. Mag.* **45,** 285 (1898).

(2) 1907. Improvements in the Hüfner type of spectrophotometer. *Proc. Phys. Soc.* **20,** 467. Reprinted in *Phil. Mag.* **13,** 481.

The 60° prism is replaced by a constant deviation prism. An addition converts the instrument to a spectro-polarimeter.

(3) 1917. The annealing of glass. *Trans. Soc. Glass Techn.* **1,** 61.

This paper refers particularly to glassware rather than optical glass. Twyman described the strain-viewer, based on the polarimeter, by which any internal stress is made visible. He put forward a law, based on experiment, that the mobility of glass doubled every 8 °C rise of temperature. Stress is relieved most rapidly at the annealing temperature (without fear of permanent distortion), and Twyman explained how to determine the annealing temperature for a particular glass.

(4) 1918. Interferometer for the experimental study of optical systems from the point of view of the wave-theory. *Phil. Mag.* **35,** 49.

An account is given of the use of an interferometer of the Twyman-Green form, for the examination of prisms, and for the examination of lenses.

(5) 1918. The Twenty-first Annual Traill-Taylor Memorial Lecture—On the use of the interferometer for testing optical systems. *Photogr. J.* p. 239.

In addition to a description similar to that in (4) of the prism and lens interferometer, examples are given of the results of testing photographic lenses, and how to deduce ways of improving them.

(6) 1919. (With F. Simeon.) Accuracy control in the manufacture of Abbe and Pulfrich refractometers. *J. Soc. Chem. Ind.* **38,** 142.

Two different policies may be pursued in making a number of scientific instruments of the same type: (*a*) to make the parts approximately, and rely upon those who adjust the instruments to produce a correct calibration, and (*b*) to make all parts within strict tolerances, so that when assembled each instrument is correct. The paper is concerned with the details of applying the second policy to the design of refractometers.

(7) 1920. An interferometer for testing camera lenses. *Trans. Opt. Soc.* **22,** 174. Reprinted in *Phil Mag.* **42,** 777 (1921).

This instrument was an improvement over that described in (4), having been modified so that it could be used to test for oblique pencils of light through the lens, as well as axial ones.

(8) 1921. (With A. J. Dalladay.) The measurement of small variations of refractive index throughout meltings of optical glass. *Trans. Opt. Glass Techn.* **5,** 325.

When making a large batch of instruments, it is important to have a large batch of glass of the same refractive index, so that a large number of similar optical components may be made. A melting of glass which has a uniform refractive index is required. To test the constancy of refractive index, use is made of a Twyman-Green interferometer. Pieces of glass are taken from each block into which the melting is cut. These pieces are ground to 1/10 in. thick and polished flat. They are then put in optical contact and adhesed, so that the pile forms a small composite block. This composite block is ground parallel and polished to ½ or 1 in. thick. A similar solid block is made, which this time comes all from one part of the melt. These are put one in each arm of the interferometer, and if the refractive index is the same throughout both blocks, the fringes will be straight. Breaks will show when portions have not the same refractive index, and the extent of the break gives the amount.

(9) 1921. (With A. J. Dallady.) Variation in refractive index near the surfaces of glass melts. *Trans. Opt. Soc.* **23,** 131.

Two pieces of glass, one from the top of a melt of dense flint, the other from the top of a melt of hard crown, were polished on two parallel faces to a thickness of 13 mm, and examined separately in the Twyman-Green interferometer. The fringes showed how the refractive index of each glass varied near the surface. At one to two millimetres from the surface, the refractive index increased sharply in the light glass, and decreased sharply in the hard glass, recovering in both cases at the surface.

(10) 1921. (With A. J. Dalladay.) The stress conditions surrounding a diamond-cut in glass. *Trans. Opt. Soc.* **23,** 165.

A diamond-cut in glass produces considerable strain. A cut was made in the surface of a block, and the stresses measured using a polarizing strain-viewer.

(11) 1922. Apparatus for controlling the annealing of glassware, and on annealing without pyrometers. *Trans. Soc. Glass Techn.* **6,** 45.

A sequel to (3). In (3), the annealing temperature was determined solely by a measurement of viscosity, although in principle a knowledge of the elasticity also was required. Fortunately the value of the elasticity does not vary sufficiently greatly to affect the practical result. A practical difficulty in the making of glassware is the achieving of the annealing temperature, especially in coke-fired kilns. A new method is described, which obviates the theoretical difficulty about elasticity, and help glassware manufacturers by getting over the temperature maintenance difficulty. A rod of glass similar to that to be annealed is bent elastically, and put with the objects to be annealed into the kiln. If on removing the elastic stress, the strained rod recovers its original shape only to the extent of one part in twenty, the rod—and the article—will be 95 % annealed. Apparatus using this principle in a convenient way is described.

(12) 1922. Notes on diagnosing causes of chords in glass. *J. Amer. Ceram. Soc.* **5,** 289.

In some cases the cause of chords arising in a glass, may be deduced by determining certain properties of the chord material relative to those of the glass itself, and combining this with a knowledge of the ingredients (and possible impurities) from which the batch was made. The properties examined are refractive index, viscosity, and coefficient of expansion.

(13) 1922. (With J. W. Perry.) The determination of Poisson's ratio, and of the absolute stress-variation of refractive index. *Proc. Phys. Soc.* **34,** 151.

The paper describes a modification of the bent beam method of determining elastic coefficients, in which the glass beam is placed in one path of a Twyman–Green interferometer, so allowing the determination of the stress-optical coefficients. Young's modulus and Poisson's ratio may be determined simultaneously.

(14) 1923. (With F. Simeon.) On the refractive index changes in optical glass occasioned by chilling and tempering. *Trans. Soc. Glass Techn.* **7,** 199.

Chilling refers to rapid heating and cooling of glass, tempering refers to slow heating and cooling. Applied to metals, these processes modify the properties of the metals. Results are reported on a dense barium crown glass and a borosilicate crown. Chilling produces a reduction of refractive index, and this may be rectified by appropriate tempering.

(15) 1923. The Hilger microscope interferometer. *J. Opt. Soc. Amer.* **7,** 635.

A modification of the Twyman–Green interferometer specially arranged for testing microscope objectives.

(16) 1926. (With J. H. Dowell.) A new automatic screw-cutting lathe. *Proc. Opt. Conv.*, p. 1.

The design and construction of a high-precision screw-cutting lathe are described. The screw is cut between dead-centres, with automatic feed. A train of only four back gears is used, two being solid on one shaft. The system used for testing is described, giving a direct calibration for the correcting-bar.

(17) 1927. Spectrum analysis by emission spectra. *School Sci. Rev.* **32,** 251.

Early history of the subject, leading up to the quartz spectrograph.

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A consideration of the similarities between radio wave transmission, behaviour of X-rays in crystals, mass spectroscopy, and optics in the narrow sense.

(20) 1930. Absorption spectrography and some of its applications to chemistry. *J. Soc. Chem. Ind.* **49,** 535.

A description of instruments suitable for the investigation of absorption bands in the ultra-violet region.

(21) 1930. (With A. A. Fitch.) The quantitative analysis of steels by spectrum analysis. *J. Iron Steel Inst.* **2,** 1.

Light from a spark between steel samples passes through a logarithmic sector on to the slit of a quartz spectrograph. The length of the lines is a measure of the intensity of the radiation in the source. From standard samples, curves are constructed for the ratio of intensity of line pairs against concentration of the element corresponding to one line (e.g. a nickel-iron pair). Composition of an unknown sample is got by interpolation. See (22).

(22) 1930. (With F. Simeon.) The logarithmic wedge sector and its use in quantitative spectrum analysis. *Trans. Opt. Soc.* **31,** 169.

Describes the design and method of use of a rotating logarithmic wedge sector, to be interposed between a source of light and the entrance slit of a spectrograph, so that points at different heights of the entrance-slit receive different exposures. This gives rise to spectral lines of different length, the longer being the more intense. The difference in length of two lines is proportional to the logarithm of the ratio of their intensities, the two lines being due to light of the same—or very nearly the same—wavelength.

(23) 1931. (With L. J. Spencer & A. Harvey.) Rapid spectrophotometry with bi-multiple spectra and a new type of wedge cell. *Trans. Opt. Soc.* **33,** 1.

A diaphragm immediately before the entrance-slit of a spectrograph consists of a vertical row of apertures, with opaque intervals equal in height to the apertures. A second row of similar apertures, staggered in height by the width of one aperture, is to the side of the first row. Thus, by sliding the diaphragm between two exposures, strips of spectrum are produced, adjacent ones belonging to different exposures. The two exposures are made through a wedge-shaped cell—in the first exposure the cell contains a solution, in the second exposure, the solvent—this allows determination of the wavelength—absorption curve of the solute.

(24) 1931. (With A. Harvey.) Validity of the Schwarzschild relation as applied to the use of the logarithmic sector. *Trans. Opt. Soc.* **33,** 1.

It is found that the Schwarzschild relation holds within the limit of experimental accuracy, in spite of the intermittancy of illumination.

(25) 1931. The Spekker photometer for ultra-violet spectrophotometry. *Trans. Opt. Soc.* **33,** 9.

Describes the design and use of a beam-splitting device made from four quartz rhombs, so that the light path from a source may be split and the two paths pass through two absorption tubes (one for solution, and one for solvent), to be brought to adjacent areas on the slit of a spectrograph. Thus two absorption spectra are obtained simultaneously, and an attenuator of the light in the solvent beam may be made to compensate for absorption in the solution beam.

(26) 1931. (With C. S. Hitchen.) Estimation of metals in solution by means of their spark spectra. *Proc. Roy. Soc.* A, **133,** 72.

An attempt to apply the solutions of metallic salts, the methods of (16) for determining the percentages of metals in the solutions.

(27) 1931. A photometric eyepiece for visual quantitative spectrum analysis. *Trans. Opt. Soc.* **4,** 176.

In this eyepiece, one views a spectrum which is split along its length—i.e. normally to the spectral lines. One half may be moved relatively to the other, so that the upper half of one line may be brought above the lower half of another line. One half spectrum may be weakened in a measured manner with an optical wedge. This allows estimates of concentration of elements in a base metal, when one line is selected for an element, and another for the base.

(28) 1933. (With A. Harvey.) Spectroscopic estimation of nickel, manganese and chromium in steels. *J. Iron Steel Inst.* **11,** 397.

Experimental arrangements as in (21), but the results were plotted as line-length differences against logarithm of percentage of the impurity. These plots were found to be nearly straight.

(29) 1933. (With G. F. LOTHIAN.) Conditions for securing accuracy in spectrophotometry. *Proc. Phys. Soc.* **45,** 643.

Calculations are made of the extent to which the various components may depart from the standard values without affecting the result beyond the allowable experimental error.

(30) 1933. A new apparatus for rapid spectrophotometry of liquids in the ultra-violet region. *Proc. Phys. Soc.* **45,** 1.

A single exposure in a spectrograph, provides all the data for plotting an absorption-curve. The method is a modification of that described in (23); two cells are used simultaneously, the one containing the solution has a reflecting edge, and is wedge-shaped, so that the thickness of solution is different at different heights. Notches are cut in the reflecting edge, so that light can only be reflected into the spectrograph at heights where there are no notches. The solvent is placed in a wedge cell with reflecting edge, having no notches. The reflecting edges of the two cells are in line with the optic axis of the spectrograph, so that alternate strips of spectra are due to the two cells, respectively·

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(32) 1936. (With F. SIMEON.) On the refractive index changes in optical glass occasioned by chilling and tempering. *J. Opt. Soc. Amer.* **26,** 121.

This note draws the attention of some American workers to (14).

(33) 1938. William Taylor. *Obit. Not. Roy. Soc.* **2,** 363-365.

(34) 1938. (With G. F. LOTHIAN & E. S. DREBLOW.) Quantitative analysis with the microphotometer. *J. Soc. Chem. Ind.* **57,** 75.

This paper describes the method, now generally adopted, of determining the concentration of an element in a sample by photographic spectra, and measuring with a microphotometer the blackening of a line representative of the element, of a line representing another element (usually the base, i.e. that present in greatest amount), and the concentration is determined from the ratio of the blackenings, referred to ratios of blackenings made by using standard (analyzed) samples. In determining the blackening, allowance may be made for the blackness of the background.

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